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THE BETTER USE OF VITAL STATISTICS IN PUBLIC HEALTH ADMINISTRATION*

By Arthur Newsholme

Since I promised to speak on the subject announced, its apparent inappropriateness to a post-prandial assembly has obtruded itself more clearly on my mind, and I must in actual fact confine myself to a desultory discussion of some elementary considerations which bear on success in the administrative use of vital statistics.

Some years ago you honored me by electing me an honorary member of your Association, and since then I have received regularly your Quarterly Publications. Their perusal furnishes ample evidence both of the great advance made in this country in statistical study, and of the increased scope and accuracy of your records; and there is evidence of this same advance in the statistical publications issuing from Washington and in many of the reports of state and city health commissioners.

It appears to me to be desirable, in the first instance, to define the position of vital statistics or biometrics in public health teaching and administration. They occupy an essentially subsidiary position, being one of several tools which the public health worker employs in his pursuit of knowledge and measurement of results. To place them on an equal footing with the fundamental sciences of biology, chemistry, and physics, and of pathology, preventive medicine, and epidemiology in public health administration or in an organized program for the training of public health officers appears to me to imply confusion of thought and of plans.

In their present stage of development in most communities, vital statistics form an excellent servant but a very bad master for the

*An address delivered at a dinner meeting of the American Statistical Association in New York City on April 15, 1921, in honor of Sir Arthur Newsholme, M.D., K.C.B., on the eve of his departure for England.

health officer. They have been invaluable in stimulating and in guiding past public health effort, and especially in directing attention to the areas of excessive general mortality, and of excessive mortality in infancy and from special definite causes of mortality; but medical certification, especially with regard to the more general and so-called constitutional diseases, is still defective, and conclusions based on municipal or national statistics dealing with this certification must be held in suspicion. Even under more definite headings we need to bear in mind the necessary limitations of statistical inference.

Mass statistics give information which is inferior in value to the information derived from the intensive investigation of individual cases — the dynamic method of study. As James Clerk Maxwell said, "however imperfect the dynamic study of man may be in practice, it evidently is the only perfect method in principle," its shortcomings arising not from a faulty method of procedure, but from the limitations of our powers. On the other hand, results obtained by the statistical method "belong to a different department from the domain of exact science."

Many inductions in causation and in the preventive or clinical treatment of disease cannot, with our present knowledge, be expressed otherwise than qualitatively, and yet they are as well established as if they could be stated quantitatively. This is especially true for inductions as to the results of some forms of sanitary and social effort. Two instances may be adduced.

It is often difficult to show any quantitative relation between the amount of sanitary and social effort, and the reduction in the death-rate from tuberculosis. This relation evidently must be one between the total prevalence of the disease and opportunities for its communication on the one hand, and the totality of measures taken or operating for its prevention on the other hand. Were it possible to measure and to weigh exactly the value and the extent of action of each factor concerned, the relation could be shown; but we do not possess adequate knowledge for this purpose. We know, for instance, that the conditions of urban life conduce to increased tuberculosis. We know also that the death-rate from tuberculosis has declined to a greater extent in cities than in country districts. Were we able to state quantitatively the influence and the extent of action of, say, overcrowding, indoor and dusty occupations, dirty habits, etc., in increasing tuberculosis, and of improvements in domestic cleanliness in habits and homes, in greater segregation of the sick, in better factory hygiene, and in higher nutrition in diminishing tuberculosis, we could measure the relative share of each of these factors in the given result in any community. Failing this, as public administrators we possess the wider outlook of

physicians and of students of social phenomena, and we are satisfied that the precise teaching of pathology, the exact scientific experience in Denmark and in this country in the elimination of tuberculosis from herds of cattle, the analogous experience with leprosy, and the facts which we possess, confirming our pathological knowledge and showing historical relation between hospitalization of tuberculous patients in different communities and reduction in the tuberculosis death-rate, all point to the conclusion that to diminish infection from open cases of tuberculosis, and especially to diminish this for infants and for children in the first five years of life, historically has been and administratively now forms the chief means for reducing the mischief done by this great enemy of mankind.

A similar difficulty arises in measuring the value of the medical and hygienic work of health centers and of public health nurses in reducing infant mortality. Did we possess quantitative knowledge of the value of each factor influencing infantile health and disease, and were we able to measure the extent of action of each factor, it would be possible to assess the relative importance of each factor concerned in producing or preventing infantile sickness and mortality. But we usually cannot do this. It is not difficult to show curves of infant mortality for different cities, almost exactly parallel in a long series of years, in some of which health centers and public health nurses have been actively at work, while in others their number has been few or there has been none. Furthermore, such curves are not unnaturally flourished before us by economists, as well as by biometricians, as prima facie evidence of the inutility of the educational and medical work of physicians and nurses in child welfare work. But these confident assertions leave us quite cold! Were all the gross mass figures which could be marshalled to point to the conclusion that ignorance is no worse than knowledge, and that domestic cleanliness is not worth while, we should very properly doubt their accuracy or their teaching, not the merits of sanitary education and practice. In actual fact we can by comparison of carefully selected areas in different towns show the value of sanitation and instruction in reducing infant mortality, although—inasmuch as with these two factors other factors are also concerned for which accurate information cannot be tabulated—we are in most instances up against the impossibility of measuring satisfactorily the value of each single factor. In short, in a large part of public health work we must content ourselves with the fact that our work is on the lines which our knowledge of physiology, pathology, and medicine—the institutes of medicine—shows to be important, and reserve to the future the possibility of securing non-fallacious measurements of complex social influences.

Although the examples just cited display my unwillingness to submit to the sole arbitrament of arithmetical standards in the measurement of results of public health work, when pathology and medicine give accurate guidance, I would not convey the impression that vital statistics fail to give most valuable guidance in public health work; they are, indeed, indispensable for such work.

I have always regarded vital statistics as a means to an end, and not an end in itself; and my own first acquaintance with the subject arose out of the need to study these statistics, to which in the third quarter of the last century there was no English guide extant. The publication of my *Elements of Vital Statistics* led to personal contact and correspondence with medical statisticians in many countries—a relationship which became more extensive and more valuable as epidemiological investigations on successive subjects formed my fascinating task.

I recall, for instance, investigations which I undertook into pandemics of rheumatic fever and of diphtheria, in the first of which scarcely any records from America were available; but in diphtheria there was ample evidence from this country showing the natural tendency of diphtheria to be more extensively continental than insular in its incidence, and for its endemic prevalence to be on such a scale as to disguise partially the epidemic characteristics of the disease.

At a later period I recall an international inquiry in which Dr. Stevenson, now the distinguished medical statistician of the English Government, and I were engaged, in which it was desired to obtain corrected birth-rates for different cities, making arithmetical allowance for the proportion of married women in the population and for their age. And it will not be surprising to some of you to learn that the most valuable records available in America were those compiled by Dr. C. V. Chapin for the city of Providence, Rhode Island, records which with similar records from Boston showed that the American born in these cities had reached a position as regards fertility almost as low as that of Paris.

During the last ten years the value and scope of American vital statistics have greatly increased; and in addition to the important publications of the federal and state governments, your literature has been enriched and the borders of knowledge extended by the publications of many workers, including Dr. Hoffman, of the Prudential Insurance Company, and Dr. Louis Dublin, of the Metropolitan Life Insurance Company. To both of these gentlemen I have been indebted for valuable references and information during the last two winters while teaching in the Johns Hopkins University.

Your death-registration area and your birth-registration area have become extended, and the accuracy and completeness of registration have increased, though it is evident that much more needs to be done; and it remains an anomalous fact that in a community which prides itself, and rightly, on its business capacity, and which makes almost a religion of efficiency methods, there is a serious lack of driving power to secure from every community throughout the states the accurate data of births, sickness, and deaths without which periodic vital stock-taking is impracticable. In England Benjamin Ward Richardson could say many years ago in referring to Farr's epoch-making reports that it is "no longer true that pestilence walketh in the dark. It is measured and registered, walketh at last in the open day."

This is becoming more nearly true for the United States, though in many areas there is still inaccurate certification of deaths and classification of the returns in statistical offices is still unskilled; the records of sickness also are imperfect.

A few years ago in many states the registration of births was so incomplete that it was impossible to determine whether any apparent decrease in the rate of infant mortality was real or was due in part or entirely to "drives" which had been followed by improved registration of births! This happily is being improved, but the margin of error in many areas is still a serious handicap in statistical inquiries.

Among the difficulties in the way of an accurate national system of registration of births and deaths in America may be named the following:

1. America is the subject of the vastest experiment in the world's history in mixture of races. Not only has it a large negro population and a not inconsiderable population of the red and yellow races, but Anglo-Saxons, Germans, French, Slavs, Italians, Jews, and other peoples are adding to its population year by year; and although the Anglo-Saxons still predominate, this preponderance will cease ere long unless relative race-birth-rates greatly change, and the largest element on this continent will be no longer Anglo-Saxon. Happily, the American population of the future will still possess the glorious heritage of law and language, of freedom and self-government, and of moral ideals, with which the name of Anglo-Saxon is rightly associated.

With such a conglomerate and mobile population, with problems of digestion and assimilation of foreign elements on an enormous scale, it is not surprising that registration of births and deaths, with its possibilities of measuring the relative vitality and social progress of different social elements, has been neglected. It is none the less a misfortune for humanity that accurate and complete records have not

been kept of the greatest social and sanitary experiment in the world's history.

2. The state sovereignty of each of the forty-eight states has been inimical to the compilation of accurate comparable national statistics. The Bureau of Vital Statistics in the Department of Commerce of the federal government publishes annual reports which year by year are increasing in value; but water cannot spontaneously rise above its own level, and these reports have no higher value than that of the records of each individual state. Although the international rules as to classification of deaths are well known, what certainty have we that in the majority of these states these rules are rightly followed in the allocation of deaths to their appropriate heading? It would probably be impracticable for the federal government, in view of the magnitude of the task and the separate sovereignty of each state, to arrange for a federal as well as a state analysis of every single death in the country, thus ensuring exactly comparable data. This has been done in England, thus giving an accurate check on the locally published vital statistics for every sanitary area throughout the country, and rendering these statistics of local mortality comparable with those for all other areas.

3. In the majority of states the lack of civil service conditions of appointment and tenure of office of the statistical superintendent has, I gather, meant much inefficient work. This is preëminently an appointment in which evidence of competence for the post should be demanded. The majority of state statisticians in the past have been untrained physicians, with no special interest in the compilation of accurate statistics; they were not concerned with enforcing the law of registration, and were ignorant of the principles of death certification and classification.

In the General Register Office in London there is a medical statistician, a worthy successor of Dr. Farr, who has associated with him a staff competent to undertake all the needed tabular work, and who can call in the services of actuarial consultants when required. This secures a due performance of the work of registration, tabulation of results, and commentary on the classified results. The knowledge of a fully trained scientific physician, and an adequate training in the principles of death certification and of the classification of diseases, are indispensable in collecting and in utilizing vital statistics; and many well-intentioned investigations by biometricians, so-called, have shown what ridiculous results may be published in the absence of accurate pathological and medical knowledge. Complexes are treated as individual entities; "constitutional diseases" occupy the stage, although this term has ceased to have a definite and exact meaning in modern pathology.

Similarly, "heart diseases" and "renal diseases" are used in comparisons, as though each of these complexes had a single and simple, instead of a multifarious, causation. The constitutional factors and the organic fitness of the population are the subject of pseudo-learned disquisition, in happy ignorance of the elementary facts that the majority of so-called constitutional diseases—possibly all—are due to poisoning or infections derived *ab extra*, and that cardiac and arterial diseases, for instance, owe their origin chiefly to infections, of which syphilis and rheumatic fever are far and away the most important.

Incidentally, although we may properly attach importance to the hygiene of middle life and to periodical medical examinations as means to aid in preventing, or in securing the early treatment of, illness, it is well to bear in mind that the prevention of breakdown in early and late middle life is much more wrapped up with the prevention of the infections of childhood, of adolescence, and of early adult life than with any other factor that can be named. In substantiation of this statement I need only mention tuberculosis and syphilis, which together probably are responsible for one out of every five deaths from all causes at all ages in the aggregate.

4. It is unfortunate that in some states, including, I believe, Massachusetts, Ohio, and Michigan, the registration of births and deaths is not in charge of the Public Health Department of the state; and that ready access of each health officer to the vital statistics of his area may thus be a matter of grace not of right, sometimes of belated grace.

5. As far as I can ascertain, it has not been found practicable to relegate institutional deaths to the vital statistics of the place of usual residence of the deceased. In England Dr. Stevenson and I were able to organize a successful system by virtue of which the General Register Office in London has become a clearing house, and all deaths of non-residents, institutional as well as others, are referred to the area in which the deceased lived. Prior to this, curious errors occurred. An area in which a large maternity institution existed had a terrible infant mortality; and a city which prided itself on its low tuberculosis death-rate and ascribed this low mortality to its dispensary machinery, had its tuberculosis hospital for advanced cases outside the city boundary! To what extent such blunders occur in America, I cannot say. Its dimensions and population are probably too vast to admit of a federal system of relegation of deaths of outsiders; but it might be done for each state. In passing I would deprecate the imperfect correction for deaths of visitors which was commonly practiced in England prior to the institution of the system I have indicated. Health officers very

willingly excluded institutional deaths of visitors, but were less willing to add outside deaths belonging to their area!

6. In sickness registration I had the privilege of initiating one of the most valuable reforms in England, viz., the weekly tabulation in the offices of the Local Government Board (now the Ministry of Health) of the number of cases of each of the notifiable infectious diseases in every one of the 1,800 sanitary areas in England and Wales, and their publication and distribution within three or four days to every health officer. For some fourteen years these have now been summarized in an annual return, thus presenting one of the most valuable epidemiological mines of information extant. The weekly returns of cases circulated from Washington by the Public Health Service are a step toward securing similar statistics in this country; and in this connection should be mentioned the valuable relation established between the federal and state health services by the appointment of officers of the latter as collaborating epidemiologists.

7. A striking lack in the vital statistics of the United States is with respect to housing accommodation. The decennial census returns in England enable health officers to ascertain for all cities and larger areas the number of inhabited houses in each ward, the number of families living in one, two, three, four or more rooms, the number of rooms in each of these groups in which there are on an average more than two persons per room, and whether the overcrowding is of children or of adults. This information has important social significance.

8. I do not propose to discuss the difficulties which must obviously arise federally from having the responsibility for vital statistics divided between several federal departments; nor those which arise from the county control of collection of statistics in some states. I merely state the general proposition in which all familiar with the importance of the subject and the fundamental need for uniformity of method in securing data and in classifying the data when obtained will agree, that *centralized administration on a large scale is essential if accurate and strictly comparable statistics are to be obtained.*

It is equally necessary that every health officer of every state, of every county, and of every city or smaller community should receive at least weekly full particulars of every birth and death occurring within his area, and should receive daily notifications of every case of infectious disease occurring within his area.

The preceding very imperfect review of the situation may be supplemented by some further suggestions—obvious, but none the less important—for making the statistics of each state and of every area within the state increasingly useful in the service of the community. Vital

statistics are merely a means to an end. Their value consists in the light which they can throw on the prevalence of disease and on the means for diminishing its incidence.

I have not seen a state public health report in which the possibilities of utility of vital statistics have been fulfilled, although many such reports approach this goal, and still more contain much valuable material. It is necessary to bear in mind, however, that a heap of bricks does not constitute a house; and that undigested statistical matter should preferably be filed in the statistical bureau for further reference and treatment, and not presented in expensive and repellent tables to a public which is quite properly unsympathetic to such matter.

There is need for detailed study of the vital statistics of every city and state by men who have adequate medical knowledge, who can avoid the elementary pitfalls of statistics, and who above all are enthusiastically obsessed with the public health possibilities in the careful study of local statistics. In statistical work there has been, hitherto, too much shovelling into heaps, and too little building of the edifice of public health. No elaborate mathematical work is needed for the study which I am advocating, or for ordinary public health needs; but it is necessary that there be an observance of the well-known rules of statistical accuracy and of sound reasoning, based on scientifically established pathological and medical knowledge, and made with an enthusiastic desire to *make statistics act as a pointer* to the areas in which excessive mortality or sickness exists, in order that—an intensive study of its vital statistics having been secured—a corresponding social and sanitary investigation may follow and the appropriate remedy may be applied.

Such intensive studies are of special immediate importance in relation to the *mortality of maternity, infant mortality,* and *mortality from tuberculosis.* I am assuming that the sanitary index formed by typhoid fever and the important index of domestic cleanliness which the prevalence of infantile diarrhoea supplies, are kept constantly under supervision.

Together with these two, there are no more important indices of the welfare of a community than the three named above; and it ought to be practicable to state, year by year, for every area and every subdivision of an area, *e. g.*, each ward of a city, the mortality associated with child-bearing, the total and the neo-natal infant mortality (*i. e.*, under one year old and under one month old) in terms of births, and to take public health action in accordance with the findings. Hence the essential importance of complete birth registration. And it is not reassuring to find that in the year 1916 in the New England

states there were revealed the following degrees of defective birth registration. There were no birth certificates in the case of 15.2 per cent in Connecticut, 9.5 per cent in Maine, 8.7 per cent in Massachusetts, 5.6 per cent in New Hampshire, 18.3 per cent in Rhode Island, and 5.7 per cent in Vermont, of the infants who died under the age of one year. Allowing for migration of infants from without each state, which must be small in amount, these figures cannot be regarded as satisfactory.

How much more effective would be the work of our public health nurses and at our health centers if the work were concentrated particularly on the infants in areas in which intensive statistical studies show that infant mortality is heaviest; and how much better directed and therefore more successful would be that work in the diminution of neo-natal mortality if every health officer had it forced on his attention that in some parts of his area the loss of life in the first month of extra-uterine life may be twice as heavy as in other parts; and if instead of futile observations as to the relative importance of "congenital debility," "prematurity," "marasmus," or the like in the death returns—which are mere labels of inexactitude of knowledge—and instead of the theoretical and generally unfounded assumption that deaths occurring at this tender age are unavoidable, assistance were given which would secure adequate care of the mother in pregnancy, in parturition, and during the critical first month after birth. It may of course be argued that unless we know the causes of this early infant mortality we cannot act intelligently against it. Such a statement illustrates well the difference between the arm-chair statistician and the competent physician engaged in public health work. The latter knows, for instance, that syphilis is responsible for a large share of this early infantile mortality, although it but seldom emerges in death certificates, and that its maleficent action on foetal tissues can be prevented. He knows, again, that many of the fatal conditions, which cannot as yet be expressed in exact pathological terms, do not arise when appropriate medical, nursing, and domestic assistance are provided for the expectant, parturient, and nursing mother. The statistical differences in neo-natal mortality, and it may be added in the number of still-births in different areas amply confirm this conclusion.

The definition of still-births adopted in different states varies, and apart from this there are large differences in the extent to which such births are registered; and to propose, as has been done, to state puerperal mortality in terms of total including still-births, instead of in terms of live-births, is to depart from the nearest approach to an accurate denominator for the rate hitherto available.

In conclusion let me—while expressing my appreciation of the essen-

tial role of vital statistics as an instrument in extending our knowledge of the epidemiology and social history of each disease (its social history is really a part of its epidemiology), in estimating the relative position of different communities, and in assessing the value of sanitary and social work—deprecate the statistical use of groups of diseases, of multiform and heterogeneous origin, for comparative purposes as being just as unscientific as it would be for the biometrician or the chemist to insist or even to permit himself to study the properties of hydrogen in the contents of a jar containing a mixture of this gas with half a dozen other gaseous elements.

I further press the point that vital statistics can have no higher value than that of the individual death certificates on which they are based; that no conjuring with classifications will render indefinite and dubious data other than they are; and that the only line of safety in most instances is to study each certified cause of death independently and separately.

I would, finally, emphasize the important consideration that when statistics having only a qualitative and compound value are being studied, and when the factors concerned cannot—as is so often the case—be analyzed quantitatively, we are justified in denying, indeed it is our duty to deny, the value of "biometrical" work based on such data, and in resting our inferences and our administrative action, as guardians of the public health, on a careful balance of all available evidence, giving chief place to exact physiological and pathological evidence bearing on our problem.

CERTAIN PROPERTIES OF INDEX NUMBERS*

By Truman L. Kelley, *Stanford University*

Acting upon the assumption that the significance, limitations, and general meaning of an index number lie within the grasp of a non-economist, the present writer has dared to enter the field, already so ably investigated by economists, with the present statistical contribution. The two ideas that he brings to this study, which have been but partially noted in the references given at the close of the article, are, first, the importance of knowing the probable error of index numbers and the method of their calculation; and, second, the significance of correlation between price ratios in the building up of an index and in the entry and withdrawal of items.

The price of a commodity in some one year, p^1_1 (the superscript designates the commodity, while the subscript designates the year), divided by the price of the same commodity in a second year, p^1_2, is p^1_1/p^1_2, and is called a price ratio. A composite of several such ratios purporting to portray a general relationship between prices in the two years is a price index, P_1/P_2. The fundamental concept in this is the ratio or geometric concept. Indices can be built upon many bases, but irrespective of the method of construction, the usual interpretation will involve this geometric concept. The lay reader will think that P_1 is a certain proportion of P_2, and P_2 is the inverse proportion of P_1. An index which is not reversible does not parallel the thought processes inherent in the concept "price ratio," and this more elementary concept, where reversibility is the rule, is the one by means of which "price index" is interpreted. Even writers who are quite aware that the index they are using is not reversible, use price ratios and price indices in such a way that it is obvious they expect the same sort of concept to be called up in the reader's mind; for example, "$p^1_1/p^1_2 = 122$, but $P_1/P_2 = 120$ so that, etc."

In as far as the concept P_1/P_2 is commonly of a different nature from p^1_1/p^1_2, it lies in the fact that P_1 and P_2 are averages, and p^1_1 and p^1_2 are single measures. Accordingly, to parallel customary think-

* The article herewith presented, except for certain footnotes and a few minor changes, was written before the appearance of the March, 1921, number of the Quarterly containing the abstract of Professor Fisher's Atlantic City paper. This paper and the attendant discussion have suggested certain things which might well have been included, but they have not indicated a withdrawal of any portion of the present paper. It therefore seems more convincing, as an independent contribution, to let the article stand as originally prepared than to rewrite it with a view to bearing upon the detailed questions raised by Professor Fisher.

ing, P_1/P_2 should mean a reversible proportion between averages. What an "average" is may not be so definitely established in the minds of scientific people generally as is the idea "ratio," but probably the most common concept is that of arithmetic average or mean. We therefore have the somewhat anomolous situation of P_1/P_2 calling up the arithmetic concept when dealing with the two separate elements involved in it, but the geometric concept when dealing with the thing entire. Since this mixture of concepts seems likely to persist, the writer proposes as an important test of the excellence of an index number the closeness with which the operations involved in it parallel general thinking tendencies: *First and most important, reversibility of ratio, and second, arithmetic averages involved in the parts.*

That a price index has a probable error is a fact not always recognized and not entirely obvious, for it may easily happen that the price ratios are entirely reliable. It may be possible to say that the price of cotton at a certain time was p^1_1 and at a second time p^1_2. If the price quotations are accurate, then the price ratio p^1_1/p^1_2 is a true measure. The average of several such gives P_1/P_2, which is invariable. Therefore, P_1/P_2 has zero probable error as far as being the average of these particular things, but the very combining of them involves the assumption that the index has significance beyond the particular data from which it is calculated. The only exception would be when P_1 and P_2 are determined from all the possible data. As an example, let p^1_1 be the price of coal at a certain mine at the first date, p^2_1 the price at a second mine, ... , p^n_1 the price at the last mine, and similarly for the p_2's. Then, since all the sources are involved, P_1/P_2 is the index of coal prices and has no probable error, except such as might be due to faulty quotations and calculations and could therefore, by proper care, be made negligible.

This is not the typical situation. Ordinarily but a few quotations are worked up into an index and the result taken as representative of an industry or a field. We therefore have quotations which are samplings of the prices in the industry, and the statistical methods for determining the reliability of samplings apply. The formulae for probable errors given later in this article are based upon certain assumptions, including that of random sampling, but if 25 or more per cent of the possible quotations are utilized, material error in the formulae is introduced, the true probable errors being less than those given by the formulae. It is to be understood that by probable error in an index number is meant that which arises from incompleteness of data. In the following determinations of probable errors of index numbers as given by various formulae, the attempt is to see how closely one can

approximate, by a sample, the number which would be obtained were all the possible data utilized in determining the same sort of index. The probable error indicates how closely the results from the sample may be expected to tally with the results from the whole. Should there be a constant tendency in the form of index used, systematically leading to too high or too low a value, we have a systematic error, which is entirely distinct and which is not measured by the size of the probable error.*

The reason why a few quotations can yield an index which is a close approximation to a general tendency is that there is a high correlation between the quotations included and those not included in the index but pertinent to the function being measured. If there are two hundred coal mines and quotations from a half dozen are taken, an index in close agreement with the true index based upon the two hundred may be expected, because of the high correlation between quotations at different mines. To say that there is a high correlation is not equivalent to saying that the prices at the different mines tend to approach the same level, but that they tend to maintain a uniform difference. Mine A, near tidewater, may sell at a certain price, p^1, much higher than that, p^2, at mine B, remote from a center of consumption, without indicating an economically abnormal condition in the coal trade. If p^1, p^2, and other similar measures are averaged, the probable error of this average is not given by the usual formula

$$\text{P. E.}_{\text{mean}} = .6745\frac{\sigma}{\sqrt{N}}$$

due to the heterogeneity of, and to the correlation between, the p's. As an illustration, more extreme than mine quotations on coal, let us average the following prices:

Bacon per pound	$.70
Bread per pound	.10
Potatoes per bushel	1.20
Apples per box	10.00
Average	$3.00
Standard deviation	4.06
P. E. (by above formula)	1.37

* In the tests of indices suggested later in the paper there will be found none to the effect that an index should have no bias. The reason for this is that reversibility of ratio, or change of base, which is included as one of the tests, is not possible with a "biased" index. Fisher (1921) shows that an index may possess a bias due to form and a second bias due to base value weighting, and that these may exactly neutralize each other. Such a situation would, statistically, be the same as one not involving bias.

Now, presumably, the probable error of no single one of these quotations is as great as $1.37, and the average of them all will probably fluctuate but little. There probably is positive correlation between these food prices, a rise in one generally going with a rise in each of the others. These conditions are not those under which the probable error of an average is given by the usual formula. For statistical purposes there is much to be gained by having homogeneous uncorrelated material. We can secure measures which are nearly, if not entirely, homogeneous and uncorrelated by dealing with price ratios instead of prices.*

Accordingly, if the price index showing prices in year 1 relative to year 2, called i_{12}, is given by the equation,

$$i_{12}=\frac{P_1}{P_2}=\frac{1}{N}\Sigma\frac{p_1}{p_2} \quad \text{(Index formula 1)}$$

and if the standard deviation of the price ratios is σ_{12}, the probable error of i_{12} is given by

$$P.\ E.\ i_{12}=.6745\frac{\sigma_{12}}{\sqrt{N}}. \quad \text{(Probable error of index formula 1)}$$

Let us consider another kind of index,

$$i_{12}=\frac{P_1}{P_2}=\frac{\Sigma p_1}{\Sigma p_2}. \quad \text{(Index formula 2)}$$

The complete probable error formula for this kind of index involves the correlation between the p's. (See Pearson, 1910.) The index,

$$i_{12}=\frac{1}{\Sigma_w}\Sigma\left(\frac{p_1}{p_2}\right)w \quad \text{(Index formula 3)}$$

will be more reliable than formula 1 if the weights, w, used are exactly or approximately proportionate to the values of the commodities involved. In general, the greater the price ratio the less the consumption and vice versa, so that the distribution of the weighted price ratios will

* In one sense, both prices and price ratios are very highly correlated, but these correlations have quite different statistical consequences. As the price of coal at mine A approaches p'_1, due to correlation the price at mine B approaches what may be a very different value, p''_1; but as the ratio, p'_1/p'_2, from the quotations of mine A approaches, as time changes, the value ρ, due to correlation, the ratio of the quotations from mine B may be expected to tend toward the same value ρ. (The rigorous proof of this statement would be necessary before the present treatment and statement of probable errors can be considered final. Whatever error is involved is of a conservative nature, as it almost certainly would tend to make the obtained probable errors too large.) Although correlation between prices tends to throw ratios together, it tends to keep prices apart. If, therefore, we deal with ratios, the effect of correlation has already operated upon the measures used, making the distribution of ratios more homogeneous, and as a consequence making the mean more reliable. In other words, the standard deviation of the ratios of prices at date 1 to those at date 2, σ_{12}, is reduced from what it would be were there no correlation between prices, so that by this very reduction, the probable error formula when applied to ratios takes account of the correlation between prices at two different dates. For a rigorous approach to the question of probable error of a ratio see Pearson (1910 and 1911).

have a smaller variability than the distribution of price ratios alone. If $w = p_2q_2$, the value of the transactions in year 2, the formula becomes

$$i_{12} = \frac{\Sigma p_1 q_2}{\Sigma p_2 q_2}. \quad \text{(Index formula 4)}$$

Formula 4 is but a type of formula 3. It is undoubtedly more reliable than either 1 or 2, but there are too many variables involved for the writer to attempt a calculation of its probable error based upon the data for two dates only. If, however, the commodities are divided into random halves and indices determined from each half, the correlation between these sub-indices may be calculated, and from it the probable error of the total index may be obtained, as follows:

Let there be n commodities, equally excellent as representative of the whole field, which are built up into the index i. In order to determine the probable error of i we may first build up two indices, A and B, each based upon a random half of the commodities. Calculation of A and B for a number of dates will give two series, the correlation between which may be found. In doing this it is desirable that the time interval between successive indices be sufficient to insure the relative independence of the commodity quotations involved. Just as the average of the prices of bread on January 1 of a certain year and on December 30 of the same year will in general give a truer average yearly price than the average of the prices on June 30 and July 1, because in the former case the two quotations are nearly independent while in the latter one has practically but a single quotation, so sub-indices calculated at too short intervals of time scarcely constitute new data, but rather repetitions of old data. Were the correlation between successive quotations known, practical limits could be set giving periods shorter than which it would not be worth while to calculate sub-indices. Having r, the Pearson product-moment coefficient of correlation, between these sub-indices, ordinarily called the reliability coefficient, we may infer the reliability coefficient, R, of the entire index, i. This is a measure of the extent to which the index i would be expected to correlate with a second similarly derived index, and is given by the formula (Brown, p. 102, 1911),

$$R = \frac{2r}{1+r}. \quad \text{(Formula to infer the reliability of a measure knowing the reliability of its halves)}$$

The probable error of i is then given by*

$$P.E._i = .6745\sigma'\sqrt{1-R}$$

* The correlation between the index i and the "true" index, where the true index is defined as the average of a very large number (an infinite number) of such indices as i, is $\sqrt{R}$. (For proof see Kelley, 1916.) Now consider a correlation table, or scatter diagram, between true index values and i-values. The standard deviation of the arrays corresponding to a certain true score, according to the usual

in which σ' is the standard deviation of the indices i for the same period of time as covered by the correlation table giving r. If σ equals the average of the standard deviations of the two series of sub-indices (presumably these two standard deviations are very nearly equal) then*

$$\sigma' = \sigma \sqrt{\frac{1+r}{2}}.$$

Substituting the values found for R and σ', the probable error formula may be rewritten,

$$P.\,E._i = .6745\,\sigma \sqrt{\frac{1-r}{2}}.$$ (Probable error of an index in terms of the correlation and standard deviation of sub-indices)

Note that r and σ must be obtained from the same series of sub-indices.

The practical advantages of reporting two sub-indices as well as the total index may well be as great as has been found to be the case in reporting two comparable measures in the fields of psychology and education. The probable error of any index may be determined if comparable sub-indices are calculated and if the series of indices covers a sufficient length of time to yield a reliable measure of correlation between sub-indices. Probably 16 pairs of quarterly sub-indices would suffice.

Accordingly, a second important measure of the excellence of an index number is *the size of its probable error.*†

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formula for the standard deviation of an array (see Yule, 1912) is, $\sigma_{i,I} = \sigma' \sqrt{1-(\sqrt{R})^2}$, in which σ' is as defined above and $\sigma_{i,I}$ is the standard deviation of the i's for a given true value of the index. Thus $\sigma_{i,I}$ is simply the standard error of the index i, and the probable error is .6745 times as great.

* Let σ_1 = the standard deviation of the A series of indices, σ_2 of the B series, and let $i = (A+B)/2$. Let Δ stand for a deviation (an error as judged by the true index) in the i index, δ for one in the A index, and d for one in the B index, then,

$$2\,i = A + B$$
$$2\,\Delta = \delta + d$$

Squaring, summing, dividing by N, the number of cases, and noting that the sum of the δd products equals $Nr\sigma_1\sigma_2$, yields,

$$4(\sigma')^2 = \sigma_1^2 + \sigma_2^2 + 2r\sigma_1\sigma_2.$$

If the standard deviations in the right-hand member are nearly equal we may replace them by $\sigma[=(\sigma_1+\sigma_2)/2]$ giving,

$$\sigma' = \sigma \sqrt{\frac{1+r}{2}}.$$

† I judge from the none too complete abstract that Fisher (1921) has calculated a large number of different indices from the same material and found that certain formulae give highly comparable results. The uniformity of indices involving the same data is not the problem of reliability here attacked. We are concerned with the problem of sampling. As to whether Professor Fisher has also compared an index determined from a part of his data with the same index as obtained from a larger part I cannot determine from the abstract, but if so it constitutes an experimental approach to the problem in hand. One would expect that the differences which Professor Fisher would find between an index based upon, let us say, ½ of his data and one based upon the remaining ½ would be somewhat larger than implied by the formulae here given, as the index based upon the ½ would be a fallible standard. A study of the uniformity of indices based upon the same data throws light upon the existence and the nature of systematic tendencies, or biases, but none whatever upon the error of sampling.

Space will not permit a discussion of the probable errors of all the proposed types of indices, but to point out the necessity of such discussions the writer has made an estimate, after more or less complete mathematical analysis, of the relative size of the probable errors of the index numbers given in the table on pages 836–7.

The one that seems the most reliable of all, and that also most completely meets other conditions except that of paralleling general thinking tendencies, is the weighted geometric mean index, in which the weights are roughly proportional to the reliabilities of the price ratios. This requirement as to weights is practically no limitation at all, as it is regularly approximated to by customary weighting devices. Practically without exception the observations of Mitchell (1915) as to what items to include in an index and what weights to give, are statistically equivalent to weighting price ratios according to reliability. As soon as a commodity becomes archaic the proper thing to do is to withdraw it, and withdrawals and entrances are readily accomplished with the geometric index. The weighted geometric mean index formula is

$$i = \sqrt[\Sigma w]{\frac{(p^1{}_1)^{w_1}\,(p^2{}_1)^{w_2}\ \ldots\ (p^n{}_1)^{w_n}}{(p^1{}_2)^{w_1}\,(p^2{}_2)^{w_2}\ \ldots\ (p^n{}_2)^{w_n}}}. \quad \text{(Index formula 5)}$$

For convenience, and without any loss of generality, Σw may be made to equal 1. Thus, letting $\omega_1 = w_1/\Sigma w$, $\omega_2 = w_2/\Sigma w$, etc., and as before. $\rho_1 = p^1{}_1/p^1{}_2$, etc.,

$$i = \rho_1^{\omega_1}\,\rho_2^{\omega_2}\ \ldots\ \rho_n^{\omega_n}. \quad \text{(Index formula 5a)}$$

Note that with this formula the index is reversible and that there is complete freedom in changing the base. Assuming as before that there is no correlation between ratios, the probable error is given by

$$P.\,E._i = .6745\,\frac{i}{\Sigma w}\sqrt{\frac{w^2{}_1\sigma^2{}_1}{\rho^2{}_1} + \frac{w^2{}_2\sigma^2{}_2}{\rho^2{}_2} + \ \ldots\ \frac{w^2{}_n\sigma^2{}_n}{\rho^2{}_n}} \quad \text{(Probable error of the weighted geometric mean index)}$$

in which the ρ's are successive price ratios and the σ's their standard deviations. As an approximation, the σ's may be considered to be equal to each other and to equal the standard deviation of the distribution of price ratios. In order that this probable error remain small, it is necessary that no one of the ratios w_1/ρ_1, w_2/ρ_2, etc., be exceptionally large.

$$\frac{w_1}{\rho_1} = \frac{w_1 p^1{}_2}{p^1{}_1}.$$

Letting q^1_1 equal the quantity of the commodity consumed, or in trade, it would be expected that $q^1_1 p^1_1$ would fluctuate much less than p^1_1, and whereas there might be danger of p^1_1 becoming extremely small or large there is not equal liklihood of $q^1_1 p^1_1$ doing so. Accordingly, if w_1 is approximately $= q^1_1 p^1_1$, then $w_1/\rho_1 = q^1_1 p^1_2$, a magnitude which is not likely to be extremely large. However, should a commodity change greatly in its relative importance, the weighting of it may easily be changed as follows:

Let it be desired to change the weight of the price ratio ρ_1 from w_1 to W_1, which we will say is a smaller weight. We need not impose the condition that $\rho_1 = i$. For $\rho_1 > i$ we will search the list of price ratios for (a) a ratio $> i$ which is underweighted, or (b) a ratio $< i$ which is overweighted. Suppose ρ_2 is such a ratio. Ordinarily there are a number of price ratios $= 1.0$, or i, or some other value which is the modal value. These may be combined and represented by ρ^s, where ρ is this modal value and s the sum of the weights of all the ratios having this value ρ. Letting P stand for the product of all the terms other than ρ_1, ρ_2, and the ρ terms, we have

$$i = \sqrt[\Sigma w]{\rho_1^{w_1} \rho_2^{w_2} \rho^s P}$$

and it is desired to change this to

$$i = \sqrt[\Sigma W]{\rho_1^{W_1} \rho_2^{W_2} \rho^S P}.$$

The first index will equal the second in case

$$w_1 + w_2 + s = W_1 + W_2 + S \tag{1}$$

and also,

$$\rho_1^{w_1} \rho_2^{w_2} \rho^s = \rho_1^{W_1} \rho_2^{W_2} \rho^S, \tag{2}$$

or, taking logarithms,

$$w_1 \log \rho_1 + w_2 \log \rho_2 + s \log \rho = W_1 \log \rho_1 + W_2 \log \rho_2 + S \log \rho.$$

W_1 is the new weight that has been assigned (this may be zero) so that everything involved is known except W_2 and S, and the solution of the two equations simultaneously will yield these. Ordinarily S will differ but slightly from s, and W_2 will differ from w_2 in the direction in which it is desirable it should differ. Thus, as a practical matter, the weight of any price ratio, whether equal to i or not, may be changed without affecting the index.

No other index, as far as the writer can determine, offers the extreme flexibility in changing weights, dropping or adding new items, here found to exist for the geometric mean index. Since this is so, the

weights can be made such that extreme ratios are given small weights or eliminated. As a consequence, the probable error of such a weighted geometric mean index may be expected to be smaller than that of any other index mentioned. The excellence of this index seems to the writer so great as to warrant its use, even though it involves a change in the established habits of interpretation of the usual reader.

Two criteria, the paralleling of habitual modes of thinking and reliability, have been proposed in judging the excellence of an index measure. Fisher (1913) has used eight other tests, three of them being tests only of "trade" indices. It would seem that these latter would be of particular importance only in case an index ceases to be a sampling and becomes an expression of the sum total of transactions involved. The table on page 836–7, in part taken from Fisher (1913), gives "scores" of the most important index measures upon several tests or criteria of excellence.

Test 1: Reliability. In giving scores upon this point the writer has freely used his judgment in the case of indices for which no simple probable error formula is available. More or less complete statistical analysis has preceded this scoring, but it is in no sense to be considered final. An "s–i" after a score means that no simpler way for calculating the probable error than by means of the correlation between comparable sub-indices seems to be available. As the writer judges this test to be the most important of all, the scoring is 3, 2, 1, and 0, instead of 2, 1, and 0—the larger the score, the higher the rating.

Test 2: Parallels habitual modes of thinking. Score 2, 1, 0.

The following tests are from Fisher.

Test 3: Proportionality. "A price index should agree with the price ratios if these all agree with each other." Stated algebraically:

$$\text{Given } \frac{p^1_1}{p^1_2} = \frac{p^2_1}{p^2_2} = \text{etc.} = i. \quad \text{Required that } \frac{P_1}{P_2} = i.$$

Score of 2 if true for any two years. Score of 1 if true only when year 2 is the base year.

Test 4: Entry and withdrawal. A price index should permit the entry and withdrawal of price ratios without changing the value of the index. Fisher uses a less general test: "A price index should be unaffected by the withdrawal or entry of a price ratio agreeing with the index." The scoring here follows Fisher, except for formula 5,

which Fisher does not include in his list of 44, and for formulae 14 and 15 which are here scored higher than by Fisher.* Score 3, 2, 1, 0.

* Fisher scores both of these formulae zero on the basis of entrance and withdrawal of items. It is, however, a simple matter to enter or withdraw price ratios, if the proper choice of weights is made. Proof for formula 15 is as follows:

To simplify notation, let

$$a = \Sigma p_1 q_1$$
$$b = \Sigma p_2 q_1$$
$$c = \Sigma p_1 q_2$$
$$d = \Sigma p_2 q_2$$

Then,

$$i = \sqrt{\frac{\Sigma p_1 q_1}{\Sigma p_2 q_1} \times \frac{\Sigma p_1 q_2}{\Sigma p_2 q_2}} = \sqrt{\frac{ac}{bd}}. \quad \text{(Index formula 15)}$$

Consider first the case of entering a price ratio which agrees with the index,

$$\rho_1 = \frac{p^1_1}{p^1_2} = i,$$

and let it be desired to enter it with the weight q_1 in year 1. It is required to determine q_2 so that this new price may be included without changing the value of the index. Since $\frac{p^1_1}{p^1_2} = \frac{p^1_1 q^1_1}{p^1_2 q^1_1} = \frac{\sqrt{ac}}{\sqrt{bd}}$ therefore $p^1_1 q^1_1$ is equal to some constant, k, times $\sqrt{ac}$, and $p^1_2 q^1_1$ is equal to the same constant times $\sqrt{bd}$, so we may write,

$$\left.\begin{aligned} p^1_1 q^1_1 &= k\sqrt{ac}, & p^1_1 q^1_2 &= j\sqrt{ac} \\ p^1_2 q^1_1 &= k\sqrt{bd}, & p^1_2 q^1_2 &= j\sqrt{bd} \end{aligned}\right\} \quad \text{from which } \frac{q^1_1}{q^1_2} = \frac{k}{j}.$$

Introducing the new price, we have

$$\sqrt{\frac{a+k\sqrt{ac}}{b+k\sqrt{bd}} \times \frac{c+j\sqrt{ac}}{d+j\sqrt{bd}}} = \sqrt{\frac{ac}{bd}}.$$

It remains only to solve this for k in terms of j to find the ratio which must hold between q^1_1 and q^1_2 to enable introducing the price without changing the index. Algebraic reduction gives

$$\frac{k}{j} = \frac{ab(di-c)}{cd(a-bi)} = \frac{q^1_1}{q^1_2}.$$

We may therefore introduce a new commodity whose price ratio agrees with the index provided the quantities or weights are in the proportion shown. The following example is given as an illustration of the entirely reasonable weightings obtained. Here the $p^1_1 q^1_1$ entered is large, being approximately one-tenth of the $\Sigma p_1 q_1$ term. Given

$$i = \sqrt{\frac{1800}{2000} \times \frac{2200}{2500}} = .889944$$

$$p^1_1 = .20, \qquad q^1_1 = 1000$$

$$p^1_2 = .2247333, \; q^1_2 \text{ is required.}$$

Solution gives $q^1_2 = 1236$, a very reasonable answer, as the ratio $\Sigma q_1 / \Sigma q_2$ must very nearly equal 1000/1236, as it can hardly differ greatly from a/c or b/d, which equal $\frac{1000}{1222}$ and $\frac{1000}{1250}$ respectively.

The principle illustrated may be followed in introducing prices whose price ratio is not equal to the index. In such case k is not a rectilinear function of j, and if the introduced price ratio differs much from i quite absurd weightings might be required in order to preserve the value of the index. However, excepting only the weighted geometric mean index, this index lends itself the most readily to the introduction or withdrawal of items. The formula should be especially serviceable for chain indices.

$$i_{12} = \frac{\frac{\Sigma p_1 q_1}{\Sigma p_2 q_1} + \frac{\Sigma p_1 q_2}{\Sigma p_2 q_2}}{2}. \quad \text{(Index formula 14)}$$

By a procedure similar to the preceding for formula 15, it is found that a price ratio, equal to the index, may be introduced without changing the value of the index, if the weights or quantities q^1_1 and q^1_2 are in the ratio $\Sigma p_2 q_1$ to $\Sigma p_2 q_2$.

SCORES OF INDEX NUMBERS UPON BASIS OF SIX TESTS OF EXCELLENCE

Tests	(1) Type IA $\frac{1}{N}\Sigma\frac{p_1}{p_0}$ Carli Evelyn Economist Sauerbeck Soether	(2) Type IA $\frac{\Sigma\frac{p_1}{p_0}w}{\Sigma w}$ Young Falkner Dun	(3) Type IA $\frac{\Sigma\frac{p_1}{p_0}p_0q_1}{\Sigma p_0q_1}$ Palgrave	(4) Type II Median value of $\frac{p^1_1}{p^1_0}, \frac{p^2_1}{p^2_0}\ldots$ Edgeworth	(5) Type II Weighted median	(6) Type III also Type V $\frac{\sqrt[n]{p^1_1p^2_1\ldots}}{\sqrt[n]{p^1_2p^2_2\ldots}}$ Jevons Westergaard	(7) Type III also Type V Weighted geom. mean	(8) Type IV $\frac{\Sigma p_1}{\Sigma p_2}$ Bradstreet
1 Reliability Smallness of P. E.	.5s-i	1.5s-i	1. s-i	2.	3.	1.	3.	1.5s-i
2 Parallels habitual mode of thinking	1.	1.	1.	1.	1.	.5	.5	2.
3 Proportionality	2.	2.	1.	2.	2.	2.	2.	2.
4 Entry and withdrawal	2.	2.	1.	1.–	1.–	2.	3.	2.
5 Change of base	.0	.0	.0	1.–	1.–	2.	2.	2.
6 Change of unit of measurement	2.	2.	2.	2.	2.	2.	2.	.0
Totals	7.5	8.5	6.0	9.0–	10.0–	9.5	12.5	9.5

SCORES OF INDEX NUMBERS UPON BASIS OF SIX TESTS OF EXCELLENCE—*Continued*

Tests	(9) Type IV $\frac{\Sigma p_1 w}{\Sigma p_0 w}$ Lowe	(10) Type IV $\frac{\Sigma p_1 \frac{q_0+q_1}{2}}{\Sigma p_0 \frac{q_0+q_1}{2}}$ Edgeworth Marshall	(11) Type IV $\frac{\Sigma p_1 \sqrt{q_0 q_1}}{\Sigma p_0 \sqrt{q_0 q_1}}$ Scrope and Walsh	(12) Type IV also Type IA $\frac{\Sigma p_1 q_0}{\Sigma p_0 q_0}$ Scrope Sidgwick Sauerbeck Giffen	(13) Type IV also Type IH $\frac{\Sigma p_1 q_1}{\Sigma p_0 q_1}$ Scrope Sidgwick Sauerbeck Giffen	(14) Type VI Arith. average of (12) and (13) Sidgwick Drobisch	(15) Type VI Geom. average of (12) and (13)	(16) Type V $\frac{\Sigma p_1 q_1}{\Sigma q_1} \Big/ \frac{\Sigma p_2 q_2}{\Sigma q_2}$ Drodisch Rawson-Rawson	(17) Type V $\frac{\Sigma p_1 q_1}{\Sigma p_2 q_2}$ $\sqrt[n]{q^1_1 q^1_1 \ldots} \Big/ \sqrt[n]{q^1_2 q^1_2 \ldots}$ Nicholson Walsh
1	2.5s-i	2.5s-i	2.5s-i	2. s-i	2. s-i	2.5s-i	2.5s-i	2. s-i	2. s-i
2	2.	1.5	1.5	1.	.5	.5	1.5	.5	.5
3	2.	1.	1.	2.	1.	1.	1.	.0	.0
4	2.	1.	1.	2.	1.	1.	1.	.0	.0
5	2.	1.	1.	.0	.0	.0	1.	2.	2.
6	2.–	2.	2.	2.	2.	2.	2.	.0	2.
Totals	12.5–	9.0	9.0	9.0	6.5	7.0	9.0	2.5	4.5

Type IA: Arithmetic average of ratios
Type IH: Harmonic average of ratios
Type II: Median of ratios
Type III: Geometric average
Type IV: Quotient of aggregates
Type V: Quotients of functions of data of single years
Type VI: Composites of preceding types

Test 5: Change of base. "The ratios between price indices should be unaffected by reversing or shifting the base." Algebraically stated: Let $i_{12}=\frac{P_1}{P_2}$, $i_{45}=\frac{P_4}{P_5}$, etc. Required that $\frac{i_{34}}{i_{14}}=\frac{i_{32}}{i_{12}}=\frac{P_3}{P_1}=i_{31}$. Give score of 2 if true for any two years, score of 1 if only true when the base year and one other is involved, *i. e.*, if only such equations as $\frac{i_{33}}{i_{13}}=i_{31}$, $\frac{i_{22}}{i_{42}}=i_{24}$, etc., hold.

Test 6: Change of unit of measurement. "The ratios between various price indices should be unaffected by changing any unit of measurement." Score of 2 or 0.

Fisher has a "Determinateness" test which he describes in the words, "A price index should not be rendered zero, infinity, or indeterminate by an individual price becoming zero." This is but one phase of reliability and is therefore included in Test 1 above.

In the formulae listed the q's stand for quantities of commodities consumed or in trade and are weights of the p's. When weights not exactly equal to the q's are involved, the symbol w is used. It is of course assumed that care would be exercised in selecting these weights w. p_0 and q_0 instead of p_2 and q_2 are used in those formulae in which the treatment of the data for the base year is unique. Test 5 is not completely met by any such formulae.

Formulae 7 and 9, which are given the highest scores, involve weights, w, instead of quantities, q. There is great flexibility in each of these so that if a weight is adopted, let us say in the first instance upon the basis of quantities (if using formula 9) or values (if using formula 7) in trade, which tends to become unreasonable, it can be changed without affecting the index between the year when the change is made and the preceding year. If years from early to late are designated by 1, 2, 3, 4 and if a formula 7 index number is started at the end of the first year, using weights proportionate to the values of the commodities in trade, and continues until the beginning of year 4 before a change in weights is desirable, a change can at that time be made which will preserve the index i_{34} and its reciprocal i_{43}. The new weighting would probably give an i_{42} and an i_{41}, were they to be calculated, which would be slightly different from those given by the equations:

$$i_{42}=\frac{i_{43}}{i_{23}} \text{ and } i_{41}=\frac{i_{43}}{i_{13}}$$

which would exactly hold had no change in weights been made. This difference will usually be small, but if an index permitting changes in weightings and at the same time enabling the use, without approximation, of any year as base is demanded, it may be made by the expenditure of a little more labor.

Formula 12 (or 13) in which there are no parameters, or flexible weightings, will serve as a foundation:

$$i_{12} = \frac{\Sigma p_1 q_2}{\Sigma p_2 q_2}.$$

Let M_1 = the mean of the p_1's

m_1 = the mean of the q_1's

S_1 = the standard deviation of the p_1's

s_1 = the standard deviation of the q_1's

r_{11} = the correlation between the p_1's (represented by the first subscript) and the q_1's (represented by the second subscript).

Symbols with other subscripts have comparable meanings, *e. g.*, r_{24} = the correlation between the p_2's and the q_4's. Then,

$$\Sigma p_1 q_2 = N(M_1 m_2 + r_{12} S_1 s_2)$$
$$\Sigma p_2 q_2 = N(M_2 m_2 + r_{22} S_2 s_2).$$

Consequently, the numerator and the denominator for the index between any two years may be built up if the means, standard deviations, and correlations are known. The data required may be calculated each year, as the data for the years become available, and tabulated in such a table as the following:

DATA FOR DETERMINING INDEX WITH ANY DESIRED YEAR AS BASE

Years		M_p	M_q	$-S$	s	rpq: p for year indicated in stub and q for number of years indicated earlier (−) or later (+)											
						−32	−16	−8	−4	−2	−1	0	+1	+2	+4	+8	+16	+32
1919	9	+		+						+								
1918	8																	
1917	7	*	+×*	*	+×*							*						
1916	6																	
1915	5																	
1914	4																	
1913	3																	
1912	2												·					
1911	1	×		×											×			

If it is desired to make 1917 the base and to express the prices in 1919 and 1911 relative to it, then $\Sigma p_9 q_7$ is determined from the magni-

tudes recorded in the compartments in which there is "+"; $\Sigma p_1 q_7$ from the compartments in which there is "×"; and $\Sigma p_7 q_7$ from the compartments in which there is "*."

The table as drawn up does not provide space for all the possible correlation coefficients. With such care as could be taken in choosing the units of quantity, the correlation coefficients could be made to vary from year to year in a very regular manner, thus enabling interpolation with high accuracy. There is complete freedom in changing the weights of commodities, but it should be noted that a commodity "dropped" continues as one of zero price and zero quantity—in other words, the N has not been decreased by "dropping" the commodity. To change the weight of a commodity price from w to w' demands a warrant. Let us say that such warrant is found in the ratio of the quantities consumed. No less warrant is necessary when w' is zero. An article once included in the index should come out only in case it becomes practically obsolescent. No distortion of any index would result in this case. We may of course take out a commodity under other conditions without affecting some one particular index.

Formula 13, particularly serviceable if trade indices are involved, may be derived from the table of constants giving a formula 12 index.

The number and nature of the commodities entering into an index is a function of the accuracy required and of the particular purpose to be served by the index. Ruling out of consideration the index which is based upon a complete survey of a field the question is, what are the principles which should control in drawing a sampling? The fundamental principles of multiple correlation apply—high correlation with the purpose to be served and low intercorrelation. If a coal price index is being constructed from a small number drawn from a much larger number of quotations, the quotations should be chosen so that (a) each is as little correlated as possible with the other quotations included in the index, and (b) each is as highly correlated as possible with the other quotations in the field not included in the index. It is to be expected that commercial tendencies will conspire to prevent any quotation from markedly possessing both characteristics, in which case a balance must be struck between them: (b) is the more important if the number of quotations in the index is mall, say not over six, but (a) is by far the more important if the number of quotations is large. In fact, quotations that are excellent for incorporation in an index number based upon a small number of items may be expected to be relatively inferior for incorporation in an index based upon a large number of items. This brief observation as to the significance of correlation between commodity prices is, in the main, an addendum to,

not in opposition to, the points involved in Dr. Mitchell's (1915) very thorough exposition of the question of what commodities should be included.

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THE MEASUREMENT OF THE RELATIVE ECONOMIC STATUS OF FAMILIES

By Edgar Sydenstricker and Willford I. King

For many centuries people have been spoken of as being rich, well-to-do, or poor. Within broad lines, and for a given time and place, this classification conveys a distinct meaning; but it is not accurate enough for use in scientific analysis. In careful economic research it is insufficient to put into the same class a billionaire and the village banker, even though both are commonly spoken of as rich men. Similarly, the well-to-do and the poor must be divided into subclasses so that different grades of comfort or poverty may be distinguished. How can this subdivision be successfully accomplished?

The usual reply to this query is that it is easy enough to make such a comparison by using income as a criterion. This answer, however, although it appears specific enough is, in reality, extremely vague and indefinite. How is this income comparison to be carried out? How shall we compare the income of the school boy who earns nothing with that of the delivery boy who works by the month? Is the delivery boy rich and the school boy poor? It is perfectly apparent that no such condition can be assumed to exist, for the school boy's parents may be rich and may furnish him with an abundance of everything he needs, while the delivery boy may receive only bare necessities. Evidently, then, personal earnings are entirely unsatisfactory as a criterion of income, and if we would compare individuals as to well-being, we must study the income of the family as a whole and not merely that of the individuals composing it.

This important truth has, unfortunately, been ignored by certain investigators who have assumed that the man who gets good wages is necessarily of a higher income class than the man who gets poorer pay. The exceptions to such a rule are, however, so numerous as largely to invalidate all conclusions based thereon. The determination of individual earnings must, therefore, for purpose of comparing welfare, be regarded as merely a step preliminary to the computation of the net family income.

As most accountants and statisticians can testify, the computation of the family income is in itself a task of considerable difficulty, especially when the facts concerning the family are not all perfectly clear—a condition which is certain to prevail when one deals with data collected

rather rapidly from a large number of families. The following are a few of the difficulties involved:

1. One must differentiate carefully between business expenses incurred in obtaining the net income and consumption expenses paid for out of net income.
2. One must be careful to exclude the income of boarders and visitors who are not part of the economic family even though they are temporarily living under the same roof and sharing in the same food supply.
3. One must include only profits (and not gross receipts) accruing from boarders, lodgers, gardening operations, the rearing of live stock, etc.

But the correct computation of net family income is not the problem here under discussion. Let us assume, therefore, that all such difficulties have been overcome and that the family income has been accurately computed. The real problem—that of comparing families as to their respective income conditions—still awaits solution. An illustration of the difficulties involved may prove helpful. Let us first suppose that the Smiths have an income of $2,000 a year and that the Clarks have only $1,800. Which family is better off financially? Manifestly, some data is necessary before the question can be answered. If the Clarks have four children and the Smiths have six, the Smiths are probably poorer than the Clarks. The size of the family is, then, evidently a factor of prime importance.

The solution now appears simple: The Smith family consists of eight persons and the Clark family of six; therefore $\frac{\$2,000}{8} = \$250$ per capita, while $\frac{\$1,800}{6} = \$300$ per capita. The Clarks are, therefore, apparently slightly richer than the Smiths. But let us not be too quick in accepting this conclusion. The Clarks have two children who are nearly grown, attend high school, go to parties, and wear fashionable clothes, whereas the Smith children are all under twelve years of age. Are we sure now that the Clarks are in the better economic circumstances? Such a conclusion is by no means dependable. Evidently the age of the members of the family must also be considered a factor of very great importance.

Furthermore, the Smith children may be all boys and the Clark children all girls, and this fact will certainly affect the quantities of food and clothing required. Sex, therefore, is another factor which affects expenditures much too radically to be safely ignored. When all of these circumstances have been carefully considered, it no longer

appears at all easy to say which family's income enables it to supply more fully its economic needs and wants.

In 1916, when one of the present writers (E. S.) was called upon to make a detailed study of the economic conditions of the population residing in seven cotton mill villages of South Carolina, he was confronted with the need for a reasonably accurate mode of determining the relative income status of each family. Realizing that ratings made according to either total family income or per capita income would be entirely too crude for the purposes in hand, he immediately began to search for some device which would give more satisfactory results. What he needed, he decided, was a scale for each sex showing average relative total maintenance expenses for persons of different ages. No such scales, however, could be found. Nevertheless, there did exist several pairs of scales purporting to show the relative food consumption according to age and sex.

Scales of this last type were not new, for as early as 1795 Foley suggested the idea of constructing them. However, the earliest ones for which the actual records could now be found were those used by Ernst Engel in 1895. He had based his scales primarily upon the variations of individuals in weight per centimeter of height. He proposed that the food requirements of a child in its first year be selected as the unit and that this unit should be called a "quet" in honor of Quetelet.* B. Seebohm Rowntree had rated, in somewhat the same fashion, the families which he studied in York.† The scales found to be most familiar to Americans were those devised by W. O. Atwater and his associates in the United States Department of Agriculture. They were worked out from data obtained through a combination of feeding experiments and budgetary studies‡ and had, in recent years, been rather widely used. The Atwater study deals with food supply in terms of calories. The unit (known as the Adult Male Unit, and hereafter designated as the A. M. U.) is defined as the need for heat and energy from food to the extent of the average number of calories required to keep a normal young man who is engaged in moderate muscular work in a proper state of health and vigor.

Although scales showing all expenditures rather than merely the needs for food in calories were the things actually sought, still, in their absence, the feasibility of substituting therefor the Atwater food scales was deemed worthy of consideration. It was of course evident that the relative food requirements in calories were not at all the same thing as

* *Lebenskosten Belgischer Arbeiterfamilien:* Bulletin de L'Institut Internationale de Statistique, vol. ix.
† See *Poverty, A Study of Town Life*, chap. viii.
‡ See *Farmers' Bulletin* 142, p. 33, published by the United States Department of Agriculture.

relative total monetary expenditures for all consumption needs, yet it seemed that they might not differ from each other very widely after all. One would scarcely expect differences in age and sex to have an effect upon food expenses different from that which they had upon food needs in calories. It is also quite evident that food is such an important item in the family budget that it cannot fail to play a very important part in determining the distribution of all expenses. Besides, it seemed not unreasonable to suppose that the cost of other articles such as housing, fuel, light, clothing, and furniture, might be divided among persons according to age and sex in much the same proportions as is food.

On the assumption that the disparity was at least not likely to be startling, it was determined to use, during the preliminary investigation, the Atwater scale for measuring the relative sizes of the respective families as expending units. Following this plan, the fraction of an adult male unit for the given age and sex was computed for each person, the fractions thus derived for the various members of a family were added together, and the resulting sum was used as the preliminary estimate of the relative size of the family. This method was applied to all of the families studied. The total income for each family was then divided by the total number of adult male units therein and the resulting quotient, the income per A. M. U., was taken as the economic rating of all members of the household. The households were then classified into income groups having an interval of $4.00 monthly per A. M. U.

In 1917 the present writers, who were working together, both agreed that the data collected in 1916 and 1917 would furnish a good basis for the construction of scales of the type that had been vainly sought, namely, those showing the actual relative total expenditures for persons of various ages and sexes. It was determined, therefore, to attempt the derivation of such scales.*

The preliminary step was to divide total family expenditures into two parts, namely, food and other, and to work with each part separately. The first part of the main procedure was to derive scales showing the actual average relative expenditures for food for the different ages and sexes. The method decided upon was to use the Atwater scale as a starting point and to make such corrections therein as experience proved to be necessary.

Following this policy, the total food expense per week for each household was divided by the number of A. M. U.'s, which gave the food expense per A. M. U. for each household. It will be remembered that all

* For the original report of this investigation see the article entitled, "Income Classification of Families in Studying Disease Prevalence," *United States Public Health Reports*, November 26, 1920.

the families had been classified upon the basis of approximate monthly income per A. M. U. and were divided into classes having an interval of $4.00. Each income class was dealt with separately.

Within each income class the percentage of the household which was female was determined, and the families were classified upon this basis. Within each of these subclasses, the average expenditure per A. M. U. for food was determined. These averages were reduced to indices by using the average for the entire income class as a base. The indices for the separate income classes were then averaged, each being weighted according to the respective numbers of A. M. U.'s in the various income classes. The resulting index series showed a tendency to rise slightly as the percentage of the family female grew larger. This indicated the necessity of increasing the relative importance of females in making allowances for food costs. By dividing the Atwater scale for females by 0.964, the resulting quotients were such that when applied they seemed to cause the curve to conform as closely as possible to the actual relative value of food purchased for consumption by females as compared to the similar food value for males.

The adjustment for sex having been made, the next step was to find out what adjustments, if any, should be made for age. The income classification of the families as described above was retained without change. The families within each income class were next divided into three main divisions, namely, families composed of two adults and several children, families composed entirely of adults, and other families. Families in the last division were discarded as being not adapted to the purpose of this study. Families with children were subdivided according to the ages of the children. Families composed of adults were classified according to the average age of the adults. The average money value per A. M. U. of food purchased for family consumption was now compared for different ages, and it was found to be materially less in each income class for old people than for young adults, and to be slightly higher for children than the necessary food supply as indicated by the Atwater scale. A separate index series was obtained for each income class and a weighed arithmetic average of these series of indices was computed. The resulting average series for all classes furnished a series of adjusting factors to be applied to the already once adjusted Atwater scales. Multiplication by these factors produced a pair of scales which purported to show the actual relative expenditures for food for persons of different sexes and various ages.

Since these scales were derived from actual money expenditures for food rather than from the number of calories believed to be necessary for the maintenance of an individual, it was thought best to give a dis-

tinctive name to the unit upon which these original scales were based. The term chosen was *fammain*, it being an abbreviation of the phrase *food for adult male maintenance*. The fammain may be accurately defined, for any given class of people, as *a demand for food of a money value equal to that demanded by the average male in the given class at the age when the expense for his food reaches a maximum.*

It will be noticed that the fammain is quoted as a *demand for* rather than a *purchase of* food. The fammain is conceived of as a psychological attribute of the individual which continuously requires nourishment for its satisfaction. The food purchased depends upon vicissitudes of all kinds and is a most variable thing, whereas the demand is relatively constant.

As stated before, the 1916 investigation was purely preliminary and was intended largely for the development of modes of procedure. The schedules obtained were, therefore, as might be expected, incomplete in certain particulars, and the possible accuracy of the information therefrom was necessarily limited by these imperfections. For instance, the value of home-produced foods had not been calculated and added to the value of purchased foods. Such errors were, wherever possible, eliminated in the 1917 study. As a result, although it has of course been impossible to eliminate from 1917 data certain types of errors which are inseparable from an extensive budgetary study, it is believed that the errors arising in 1916 from the tentative nature of the method and from incompleteness in the calculations have been reduced until they are reasonably small.

With these improved schedules on hand, the next move was to correct the scales of fammains as derived from the 1916 study. The mode of correcting these scales was similar in concept but different in mechanical procedure from that used in making adjustments from the Atwater scale. As a material basis for the work of correction, budgets from about a thousand families were selected and actually used in the computations from which the accompanying indices are derived. These families were resident in twelve South Carolina mill villages, and the schedules were taken in the spring of 1917 before garden produce became abundant. For each family the number of fammains (computed according to the 1916 scales) was added up. The total food expense was divided by the number of fammains to obtain the expense per fammain.

It was absolutely necessary that variations in food supply owing to wide differences in income be eliminated; hence a classification of the families on an income basis was essential. Since it was entirely superfluous as well as impracticable to make this classification at all exact,

the approximate method of dividing the total weekly family income by the number of fammains was used. This income per fammain, not strictly scientific as a basis of income classification, was amply accurate for the purposes at hand.

Next, the percentage of all members who were male was ascertained for each family. The expense per fammain for food for the family was then plotted as an ordinate against the percentage of the family male as an abscissa. This was repeated for each family. A straight line was next fitted as closely as possible to the trend of the points located. The tangent of its angle with the horizontal was then measured. The tangents thus obtained showed that the fammain scales derived from the 1916 schedules still failed to allow a sufficient amount for the necessary consumption for females as relative to that of males, the fraction shown being about 3 per cent less than actual relative value of food intended for consumption. All percentages on the 1916 fammain scale for females were therefore multiplied by 1.03.

The next necessary step was to test the 1916 fammain scale for correctness as to age relationships. This was done in the following manner: All families were divided into income classes having an interval of $1.00 weekly per fammain. For each family within a given income class, the quantity representing food expenditures per fammain was plotted as an ordinate against the average age of the persons in the family. After the points were plotted, the graph sheet was divided into age classes and the median expenditures for each age class was determined. Indices were now derived by dividing the median for each age class by the median for all the age classes. This process was repeated for each income class. The indices thus derived were weighted according to the number in each subclass and the weighted arithmetic average of all the indices within a given age class was obtained for all income classes. These average indices for the various age classes, when compared, showed clearly that the 1916 fammain scales had assigned too small a rating in fammains for the older persons. The fammain scales were therefore adjusted for age as well as for sex. They now purported to show for each sex the correct rating of normal relative expenditures for food at different ages.

Since the changes made had been considerable, however, it seemed best to verify the results and to ascertain whether after the new adjustments were made, the scales actually represented the facts. The number of fammains was therefore recomputed for each of the thousand selected families according to the new scales. Again, food expenditures per tentative fammain were compared for different age classes. It was found that a small further allowance must still be made for persons of

Table I

FINAL SCALES OF FAMMAINS

Derived from Food Records of 1500 Families Collected in 1917 from Residents of Twenty Cotton Mill Villages of South Carolina.[1]

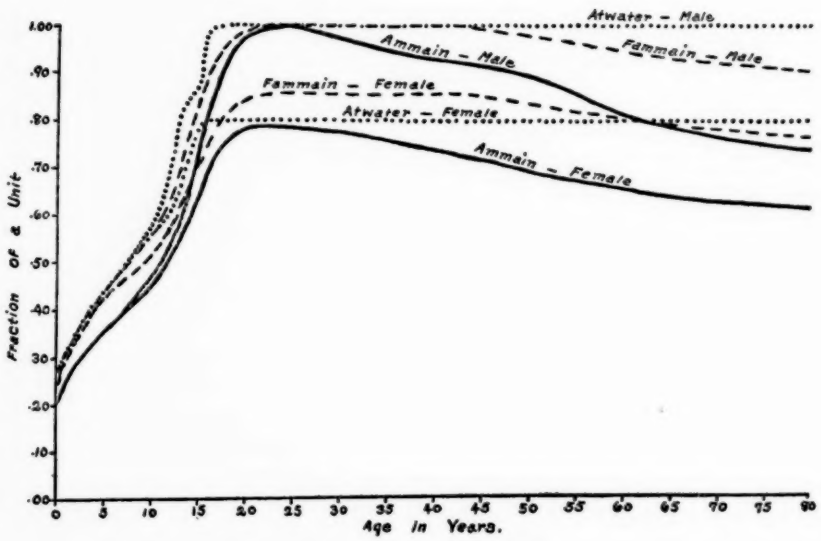
Age in Years	Male	Female	Age in Years	Male	Female
Under 1	.278	.265	40	1.000	.858
1	.302	.289	41	1.000	.858
2	.348	.335	42	1.000	.858
3	.381	.371	43	.999	.857
4	.410	.403	44	.997	.854
5	.435	.427	45	.994	.851
6	.460	.447	46	.991	.848
7	.483	.463	47	.988	.845
8	.506	.478	48	.985	.842
9	.531	.492	49	.982	.839
10	.557	.517	50	.979	.836
11	.592	.541	51	.976	.833
12	.639	.576	52	.973	.830
13	.694	.615	53	.970	.827
14	.769	.660	54	.966	.824
15	.837	.715	55	.962	.821
16	.900	.760	56	.958	.818
17	.935	.795	57	.955	.815
18	.965	.820	58	.952	.812
19	.983	.835	59	.949	.809
20	.991	.845	60	.946	.806
21	.998	.853	61	.943	.803
22	1.000	.858	62	.940	.800
23	1.000	.858	63	.937	.797
24	1.000	.858	64	.935	.794
25	1.000	.858	65	.933	.792
26	1.000	.858	66	.931	.790
27	1.000	.858	67	.929	.788
28	1.000	.858	68	.927	.786
29	1.000	.858	69	.925	.784
30	1.000	.858	70	.923	.782
31	1.000	.858	71	.921	.780
32	1.000	.858	72	.919	.778
33	1.000	.858	73	.917	.777
34	1.000	.858	74	.915	.775
35	1.000	.858	75	.913	.774
36	1.000	.858	76	.911	.772
37	1.000	.858	77	.909	.771
38	1.000	.858	78	.907	.770
39	1.000	.858	79	.905	.769
			80	.903	.768

1. The last decimal place is, in every instance, the result of interpolation and cannot be regarded as accurate.

ages above 34 years. The number of fammains was therefore increased slightly for the higher ages. The ratio between the sexes seemed to be as accurate as could be arrived at; hence no changes were required on this account. The scale as finally adjusted is believed to represent fairly the relative variation with age and sex of expenditures for food under the conditions prevailing in South Carolina cotton mill villages in the spring of 1917.

A glance at the accompanying graph shows that the fammain and the Atwater scales diverge from each other considerably in places. This

THE AMMAIN AND FAMMAIN CURVES,
REPRESENTING CONDITIONS IN THE SOUTH CAROLINA COTTON MILLS IN 1917,
AS COMPARED TO GRAPHS OF THE ATWATER SCALES



leads us to inquire whether or not this divergence is a result of the Atwater scale being based upon measurements in terms of calories, while the fammain scale is based upon money value.

It is evident that unless the food of females costs more per calorie than does that of males, or vice versa, or unless the cost of food per calorie varies with age, the calorie curve and the fammain curve must be identical. In order to ascertain whether costs per calorie actually do or do not vary with age and sex, about twelve hundred families were classified on the basis of those variables, and the food supply of each class was studied as to its calorific content. The results showed no correlation

whatever between either age or sex and the food cost per calorie. The conclusion must be that the fammain curve represents the demand for food equally well, whether it is expressed in terms of calories or of money cost. This interchangeable character makes the fammain scale doubly useful.

But food is only one of the items of the family budget. Any scale adequate for use in the accurate classification of families upon an income basis must take into consideration expenditures for other purposes as well. Now articles of different kinds can be added only when all are first reduced to a common denominator. The only common unit of measurement for food, clothing, entertainment, etc., which can be readily applied to the various articles is the dollar's worth; hence it was necessary to consider food as well as all other classes of commodities in terms of money value. It was certainly impracticable, and probably wholly useless as well, to attempt to ascertain the quantities of the various articles actually *consumed* by individuals. It proved feasible, however, to secure the housewife's estimate of the amount and value of each class of articles, except food, purchased for the various members of the family for purposes of current consumption during the preceding twelve months, and inquiries were made to this end. The ultimate object was to secure indices showing the relative purchases of the articles to be used by persons of different ages and sexes, and then to combine these indices with similar ones for food. The ordinary schedules taken in 1917 covered expenditures for food only. It was therefore necessary to undertake a special inquiry to ascertain how other expenditures were apportioned among individuals of different ages and sexes. For this purpose information was secured from some three hundred families, an estimate being obtained from some member of each family, usually the housewife, of the total expenses incurred during the preceding twelve months for each of thirty of the leading categories of purchases for the family budget, these categories including only articles of such a nature that they could readily be apportioned to individuals, as, for example, various articles of clothing, medical expense, recreation, and schooling. In addition, a statement was obtained of the family expense incurred for rent, fuel, light, and twenty-eight minor items. By use of the data thus obtained from the family, together with records secured from the merchants of the food sold from their stores to each family during sample periods, an effort was made to balance the family income and expenditures for the twelve-month period before the schedule was taken. Nearly half of the schedules failed to balance within a reasonable margin of error. All such schedules were rejected, leaving for consideration reasonably

satisfactory schedules for 140 families comprising 672 individuals. These 140 families were classified upon the basis of income per fammain (using the 1916 scale which was satisfactory enough, since for the purpose in hand it was necessary to obtain only a very crude approximation to the actual income rating of the family). The individual items for the persons of each sex within a given income class were plotted one by one, using age as an abscissa and actual expenditures for the given person's individual needs as ordinates. By a modification of the moving average method, a smoothed curve was derived for each income class from the points indicating the positions of the various items. These curves were weighted according to the number of individuals in each income class and were thereafter combined into one curve which was then smoothed. The quantities entered in Table II were read from this curve.

The index series for food was now combined with the similar series for expenditures for other articles purchased specifically for individuals, each series being weighted in proportion to the average money expended for all the individuals of each sex for food and for other articles respectively. The combined index is recorded in Table III. It takes into consideration all of the principal ordinary expenditures except those for rent (a small expense in mill villages), fuel and light, and furniture, and actually includes about 89 per cent of the total family expenditures. It seems probable that the remaining 11 per cent is apportioned in a ratio sufficiently similar to that representing the items used in deriving the scales just mentioned to make these scales fair criteria of the ordinary relative expense for maintenance of individuals of different ages and sexes among the families studied.

These scales evidently differ from the fammain scales and have a different unit as a base. The unit, in this instance, may be defined for any given class of people, as a *gross demand for articles of consumption having a total money value equal to that demanded by the average male in that class at the age when his total requirements for expense of maintenance reach a maximum.* This unit may be denominated as an *ammain.* It differs from the "adult male unit" of the Atwater scale in that it is based upon demand for all maintenance and not merely upon that for food, and in that it is a measure of *money value* of actual expenditures and not of *calories* to meet bodily needs. It differs from the fammain only in that it includes all expenditures and not merely those made for food. The term *ammain* is an abbreviation of the words *adult male maintenance.* The excuse for the coinage of the terms *ammain* and *fammain* lies only in the fact that the concepts included therein cannot be expressed accurately in ordinary English except by long descrip-

Table II

RELATIVE EXPENDITURES FOR PERSONS OF DIFFERENT AGES AND SEXES FOR ARTICLES PURCHASED FOR USE BY A SPECIFIC INDIVIDUAL

As shown by the Budgets of 140 Families in the Cotton Mill Villages of South Carolina for a Twelve Month Period Ending in 1917

Age in Years	Male	Female	Age in Years	Male	Female
			40	.81	.45
Under 1	.11	.11	41	.80	.44
1	.13	.13	42	.79	.43
2	.16	.16	43	.79	.41
3	.17	.17	44	.78	.40
4	.19	.18	45	.78	.38
5	.20	.19	46	.77	.37
6	.22	.21	47	.76	.36
7	.24	.23	48	.76	.35
8	.26	.25	49	.75	.35
9	.28	.27	50	.74	.34
10	.31	.30	51	.73	.33
11	.33	.33	52	.71	.33
12	.35	.37	53	.69	.32
13	.40	.40	54	.67	.32
14	.46	.44	55	.64	.31
15	.55	.48	56	.62	.30
16	.65	.57	57	.61	.30
17	.77	.60	58	.60	.29
18	.86	.61	59	.58	.29
19	.92	.63	60	.57	.28
20	.95	.63	61	.56	.28
21	.96	.63	62	.55	.27
22	.98	.62	63	.54	.27
23	.99	.62	64	.53	.27
24	1.00[a]	.61	65	.52	.26
25	1.00	.60	66	.51	.26
26	.99	.60	67	.51	.26
27	.97	.59	68	.50	.26
28	.95	.59	69	.49	.26
29	.94	.58	70	.49	.26
30	.92	.58	71	.48	.26
31	.91	.57	72	.47	.25
32	.89	.55	73	.47	.25
33	.88	.54	74	.46	.25
34	.87	.52	75	.46	.25
35	.85	.50	76	.45	.25
36	.84	.49	77	.45	.25
37	.83	.48	78	.44	.25
38	.83	.47	79	.44	.25
39	.82	.46	80	.43	.25

a. Average expenditure for a man 24 years of age = $100.79

tive sentences, and experience has amply proved that it is far better to coin words outright than to use existing words capable of double interpretations.

It will be observed that the scales of fammains, as might be expected, differ but slightly from the Atwater scales which represent basal food requirements in calories, while the scales for ammains are distinctly dissimilar to those representing expense for food or necessary food requirements.

Table III

FINAL SCALES OF AMMAINS

Derived From Complete Budgets of 140 Families and From Food Records of 1500 Families Collected in 1917 From Residents of Twenty Cotton Mill Villages of South Carolina.[1]

Age in Years	Male	Female	Age in Years	Male	Female
Under 1	.220	220	40	.931	.739
1	.241	.241	41	.928	.735
2	.281	.281	42	.925	.731
3	.306	.306	43	.922	.727
4	.332	.332	44	.919	.723
5	.352	.352	45	.916	.718
6	.375	.375	46	.912	.713
7	.397	.396	47	.908	.708
8	.419	.412	48	.904	.703
9	.442	.429	49	.899	.698
10	.470	.451	50	.894	.693
11	.499	.481	51	.888	.689
12	.537	.514	52	.881	.685
13	.590	.553	53	.873	.681
14	.657	.596	54	.864	.677
15	.736	.648	55	.854	.673
16	.812	.705	56	.844	.669
17	.876	.739	57	.834	.666
18	.928	.760	58	.825	.663
19	.960	.776	59	.818	.660
20	.976	.783	60	.812	.657
21	.986	.789	61	.807	.654
22	.992	.790	62	.802	.651
23	.996	.790	63	.797	.648
24	1.000	.788	64	.792	.645
25	1.000	.786	65	.788	.642
26	.996	.784	66	.784	.639
27	.990	.782	67	780	.636
28	.984	.780	68	.776	.634
29	.978	.778	69	.772	.632
30	.972	.776	70	.768	.630
31	.967	.773	71	.764	.628
32	.962	.770	72	.760	.626
33	.957	.767	73	.757	.625
34	.952	.763	74	.754	.623
35	.948	.759	75	.751	.622
36	.944	.755	76	.749	.620
37	.940	.751	77	.747	.619
38	.937	.747	78	.745	.618
39	.934	.743	79	.743	.617
			80	.741	.616

1. The last decimal place is, in every instance, the result of interpolation and cannot be regarded as accurate.

Although the ammain and fammain scales here presented are probably fairly representative of expenditures for persons of different ages and sexes among the cotton mill workers of South Carolina, it does not follow that the same relative expenses of maintenance would be strictly applicable to other classes of workers. The existence of such similarities or differences can be determined only by investigation. It might be said, however, that for the families considered in the present study, differences in income did not seem radically to affect the character of the scales. However, there were too few families of high income to afford an adequate basis for any definite conclusions as to whether extreme income variations do or do not have any tendency to modify the shape of the curves. It seems not improbable, nevertheless, that in social classes differing widely from those studied, expense for clothing for different ages and sexes might noticeably diverge from the present scales. The scales here cited, therefore, although believed to be reasonably accurate for the given class of workers in the locality under consideration, may prove somewhat inaccurate if applied to different classes of persons or to the same class in other localities.

However, for studies in which it is impracticable to work out an original scale of ammains or fammains, it is evident that these scales may be used for the purpose of improving very notably the accuracy of the family income rating over that which could be secured simply by dividing the income for the family by the number of individuals composing it or, still more crudely, by merely using the income for the family as a whole.

In fact, for studies of working-class families anywhere, if the classification according to income does not need to be extraordinarily exact, it will probably be sufficient to use the present scales as they stand.

The practical method of applying the fammain and ammain scales is shown in the following example:

SMITH HOUSEHOLD

Name	Age	Sex	Position in family	Number of fammains	Number of ammains
John	38	M	head	1.00	.94
Mary	34	F	wife	.86	.76
Fred	12	M	son	.64	.54
Oliver	10	M	son	.56	.47
Clinton	8	M	son	.51	.42
James	6	M	son	.46	.38
Harry	4	M	son	.41	.33
George	2	M	son	.35	.28
Total for family				4.79	4.12

Total annual family income = $2,000
Income per ammain = $2,000 ÷ 4.12 = $485
Total annual expenditure for food = $600
Annual food cost per fammain = $600 ÷ 4.79 = $125

CLARK HOUSEHOLD

Name	Age	Sex	Position in family	Number of fammains	Number of ammains
Clark, Samuel	45	M	head	.99	.92
Clark, Edith	41	F	wife	.86	.74
Clark, May	18	F	daughter	.82	.76
Clark, Gladys	16	F	daughter	.76	.71
Clark, Julia	13	F	daughter	.62	.55
Clark, Evelyn	11	F	daughter	.54	.48
Jones, William	28	M	boarder	1.00	
Ross, Jennie	24	F	boarder	.86	
Total for dietary group				6.45	
Total for economic family					4.16

Total annual family income =$1,800
Income per ammain =$1,800 ÷4.16 =$434
Total annual expenditure for food = $780
Annual food cost per fammain = $780 ÷6.45 =$121

We can now proceed to answer intelligently the question propounded at the beginning of the discussion as to whether the Clark family is richer or poorer than the Smith family. A comparison of their respective incomes per ammain is all that is necessary. We see that although their per capita income is slightly less, the Smith family is distinctly better off, the ratio of their income to that of the Clark family being as $485 is to $434. A comparison of the respective values of food used per fammain shows us, however, that the members of the Clark family, perhaps because they keep boarders, perhaps for other reasons, have nearly as abundant a food supply as do the Smiths. The Clarks, then, must perforce economize more diligently than the Smiths in other lines of expenditure.

This study is presented with a full realization that it is only a beginning and not a finality in this field. Its prime object is to call attention to the absolute necessity of prefacing all attempts to classify families upon an income basis by ascertaining:

1. The age and sex composition of the family.
2. The number of ammains composing the family.
3. The total net income for the family.
4. The net family income per ammain.

If it is desired to make a comparison, not according to income but according to the food supply of the persons eating at the family table, the sequence of the steps then would become:

1. The age and sex composition of the dietary group.
2. The number of A. M. U.'s or fammains composing the dietary group.
3. The total number of calories or total value of food consumed by the dietary group.

4. The number of calories per fammain consumed or the number of dollars per fammain expended for food during the period in question.

With this information at hand it is possible to proceed intelligently with further investigations requiring, as a prerequisite, knowledge of the income, dietary conditions, or general economic welfare of the families in question. Without such a systematic rating of families, the results of income or dietary studies are always affected with such a wide margin of error as to cast serious doubt upon the accuracy of any except the most strikingly evident conclusions which may be derived therefrom.

A NEW METHOD OF APPORTIONMENT OF REPRESENTATIVES*

By Edward V. Huntington, *Harvard University*

I. THE CONSTITUTIONAL REQUIREMENTS

The only provisions of the United States Constitution which bear on the present problem of apportionment are the following: "Representatives shall be apportioned among the several states according to their respective numbers, counting the whole number of persons in each state, excluding Indians not taxed." "The enumeration shall be made within . . . every . . . term of ten years." "The number of representatives shall not exceed one for every 30,000, but each state shall have at least one representative."

Since fractional representation appears to be out of the question, the problem is how to distribute any given total number (N) of representatives among the several states in proportion to their populations "as nearly as may be."

In other words, considering first the case of two states, the problem is how to approximate most closely to the condition expressed by the proportion

$$A : B :: a : b,$$

* The method here proposed was first presented at a meeting of the American Mathematical Society, December 28, 1920, and an abstract of the technical results will be published in the *Proceedings of the National Academy of Sciences*, probably in April, 1921.

The new method was brought to the attention of Congress through two letters to Chairman Isaac Siegel of the House Committee on the Census. The first (dated January 8, and published in the *New York Times* for January 16, 1921, Section VII) showed that in the apportionment bill for 483 members then pending, the relation between Missouri and Montana could be improved by changing from 16 and 2 to 15 and 3. The second (dated January 17, and published in the *Congressional Record* for January 19, 1921, page 1791) showed that in the revised apportionment bill for 435 members, which was eventually passed by the House but allowed to die in the Senate, the following improvements could be made:

	Old	New		Old	New		Old	New
New York.....	43	42	North Carolina..	11	10	Virginia.....	10	9
New Mexico.....	1	2	Rhode Island...	2	3	Vermont.....	1	2

Also, two letters to Senator Howard Sutherland, chairman of the Senate Committee on the Census, dated January 22 and 29, 1921, urged investigation of the new method, and in February Senator Sutherland referred the matter for advice to the Advisory Committee to the director of the census. The report of this committee is expected to be available before the apportionment bill is taken up afresh in the new session of Congress, which has been called for April 11, 1921.

Further publications on the mathematical aspects of this subject may be expected either in the *Transactions of the American Mathematical Society* or in the *American Mathematical Monthly*.

(The manuscript for this article was received by the editor on April 15, 1921.)

where A, B, are the populations of the two states, and a, b, the number of representatives assigned to each.

When we come to translate this proportion into the form of a numerical equation, we find that this can be done in four ways:

$$A/a=B/b;\ \text{or}\ a/A=b/B;\ \text{or}\ A/B=a/b;\ \text{or}\ B/A=b/a;$$

no one of which has any constitutional claim to be preferred over any of the others. The problem is equivalent, therefore, to the problem of how to make all four of these equations as nearly true as possible. We are thus led to the following:

FUNDAMENTAL PRINCIPLE. *In each of the four equations*

$$A/a=B/b,\ a/A=b/B,\ A/B=a/b,\ B/A=b/a,$$

the quantity on the left and the quantity on the right should be as nearly equal as possible.

This fourfold requirement may be expressed in words as follows:

(I*a*) The *number of people per representative* (that is, the size of a congressional district) in state A, and the number of people per representative in state B, should be as nearly equal as possible. $(A/a=B/b.)$

(I*b*) The *fractional share of a representative per inhabitant* in state A, and the fractional share of a representative per inhabitant in state B, should be as nearly equal as possible. $(a/A=b/B.)$

(I*c*) If state A is, say, *twice as populous* as state B, it should have *twice as many representatives*, as nearly as may be. $(A/B=a/b.)$

(I*d*) If state A is, say, *half as populous* as state B, it should have *half as many representatives*, as nearly as may be. $(B/A=b/a.)$

The validity of this fourfold fundamental principle, as far as it goes, can hardly be called in question, since, as we have seen, it is an inevitable consequence of the plain provisions of the Constitution. To make this principle precise, however, it is necessary to decide what meaning shall be attached to the words "nearly equal," and on this point there is the possibility of a difference of opinion.

II. THE PROPER MEANING OF "NEARLY EQUAL"

There are two measures of inequality in common use: (1) the *absolute* difference, as when we say, the temperatures of two rooms differ by 10 degrees; and (2) the *percentage* difference, as when we say, the wages in two cities differ by 10 per cent.

Since we are dealing here with proportions rather than with absolute numbers, we shall adopt the second of these interpretations.

DEFINITION OF INEQUALITY. *By the "inequality" between two quantities, we shall mean their percentage difference.*

For example, the inequality between 2 and 3 is 50 per cent, while the inequality between 100 and 105 is 5 per cent. Hence 100 and 105 are more "nearly equal" than 2 and 3 are.*

Applying this definition to the four cases mentioned in the fundamental principle, we readily see that any "inequality" that may exist will be precisely the same in all four cases; that is, if the quantities of any one pair are as nearly equal as possible, the quantities of each of the other pairs will also be as nearly equal as possible. In other words, on this interpretation of "nearly equal," the four parts of the fundamental principle are merely four different ways of stating one single requirement. (This would not be true on the other interpretation of "nearly equal"; see Appendix I.)

III. PRECISE FORMULATION OF THE PROBLEM

We are now in position to formulate definitely the conditions or postulates which an apportionment ought, if possible, to satisfy.

For the case of two states, we have

POSTULATE I. *For two states, A and B, the percentage difference between A/a and B/b (or between a/A and b/B, or between A/B and a/b, or between B/A and b/a, which all come to the same thing) should be as small as possible.*

For the case of three or more states one further postulate is needed.

POSTULATE II. *In a satisfactory apportionment, there should be no pair of states which is capable of being "improved" by a transfer of representatives within that pair*—the word "improvement" being understood in the sense implied by Postulate I, and the rare case of a "tie" being decided in favor of the larger state.

On the basis of these two simple postulates, the following theorem can now be established:

THEOREM I. *For any given values of A, B, C, . . . and N, there will always be one and only one "satisfactory" apportionment in the sense defined by Postulates I and II.* (See "working rule" below.)

The method thus developed may be called the *Method of Equal Proportions.*

IV. THE METHOD OF EQUAL PROPORTIONS: WORKING RULE

A practical working rule for computing the required apportionment in any given case may be stated as follows:

First, assign one representative to each state (here 48 in number).

* "Percentage difference" is here thought of as the absolute difference divided by the smaller number. For the present purpose it might equally well be thought of as the absolute difference divided by the larger number, or the absolute difference divided by the mean (arithmetic, geometric, or harmonic) between the two numbers.

Next, multiply the population of each state by as many of the numbers

$$1/\sqrt{1\times 2}, \quad 1/\sqrt{2\times 3}, \quad 1/\sqrt{3\times 4}, \quad \ldots$$

as may be necessary, and record each result, together with the name of the state, on a small card. (A table of multipliers is given below.) Then arrange these cards according to the magnitude of the numbers recorded upon them, from the largest to the smallest, thus forming a "priority list" for the given states. Finally, assign further representatives, beginning with the 49th, to the several states in the order in which the names of the states occur in this priority list, until the required total, N, has been reached.

In practice, the multiplications for any state may stop with the last result which is greater than P/N', where P is the total population of all the states, and N' is a number larger than the largest value of N likely to be required.

The proof that this working rule will always give an apportionment satisfying the conditions of Postulates I and II is a straight-forward piece of algebra which need not be given here.

It will be observed that the "multipliers" are the reciprocals of the geometric means of consecutive integers; hence the method of equal proportions may be called also the *method of the geometric mean.**

TABLE I

TABLE OF MULTIPLIERS

k	$1/\sqrt{(k-1)k}$	Diff.	k	$1/\sqrt{(k-1)k}$	Diff.	k	$1/\sqrt{(k-1)k}$	Diff.
1	....	....	18	.057 1662	.003 4677	35	.028 9886	.000 8655
2	.707 1068	....	19	.054 0732	.003 0930	36	.028 1718	.000 8168
3	.408 2483	.298 8585	20	.051 2989	.002 7743	37	.027 3998	.000 7720
4	.288 6751	.119 5732	21	.048 7950+	.002 5039	38	.026 6690	.000 7308
5	.223 6068	.065 0683	22	.046 5242	.002 2708	39	.025 9762	.000 6928
6	.182 5742	.041 0326	23	.044 4554	.002 0688	40	.025 3185−	.000 6577
7	.154 3033	.028 2709	24	.042 5628	.001 8926	41	.024 6932	.000 6253
8	.133 6306	.020 6727	25	.040 8248	.001 7380	42	.024 0981	.000 5951
9	.117 8511	.015 7795	26	.039 2232	.001 6016	43	.023 5310	.000 5671
10	.105 4093	.012 4418	27	.037 7426	.001 4806	44	.022 9900	.000 5410
11	.095 3463	.010 0630	28	.036 3696	.001 3730	45	.022 4733	.000 5167
12	.087 0388	.008 3075	29	.035 0931	.001 2765	46	.021 9793	.000 4940
13	.080 0641	.006 9747	30	.033 9032	.001 1899	47	.021 5066	.000 4727
14	.074 1249	.005 9392	31	.032 7913	.001 1119	48	.021 0538	.000 4528
15	.069 0066	.005 1183	32	.031 7500+	.001 0413	49	.020 6197	.000 4341
16	.064 5497	.004 4569	33	.030 7729	.000 9771	50	.020 2031	.000 4166
17	.060 6339	.003 9158	34	.029 8541	.000 9188	51	.019 8030	.000 4001

* This method of equal proportions resembles closely the "method of alternate ratios" proposed by Dr. Joseph A. Hill, chief statistician of the Bureau of the Census, in 1910. (See Appendix II.) In most practical cases Dr. Hill's method will give precisely the same apportionment as the method here proposed; in some cases, however, it can be shown to lead to inconsistent results. To Dr. Hill belongs the credit of being the first to propose and advocate the use of percentage differences in this problem; he also made a partial application of the idea of a priority list based on the geometric mean. In fact, the method of equal proportions, though arrived at through quite independent considerations, is in effect a carrying out to its logical conclusion of the fundamental idea of the method of alternate ratios as proposed by Dr. Hill in 1910. The writer is indebted to Dr. Hill for calling his attention to this problem in the fall of 1920, and for courteous assistance in securing advance figures on population, etc.

V. APPLICATION TO THE 1920 CENSUS

The results of the proposed method of equal proportions as applied to the 1920 census are shown in the accompanying tables. The

TABLE II

TABLES OF APPORTIONMENT (1920) FOR ANY SIZE OF HOUSE FROM 435 TO 484

1910 435	Table for 435			Priority List, showing where each additional member from the 436th to 484th should be assigned	Table for 484		
	State	1920 435	Ga in or Loss over 1910		State	1920 484	Gain over 1910
10	Ala.	10		436 N. Y. 43	Ala.	11	1
1	Ariz.	1		437 N. C. 11	Ariz.	1	
7	Ark.	7		438 Va. 10	Ark.	8	1
11	Calif.	14	3	439 La. 8	Calif.	16	5
4	Colo.	4		440 Miss. 8	Colo.	4	
5	Conn.	6	1	441 Tex. 20	Conn.	6	1
1	Del.	1		442 Okla. 9	Del.	1	
4	Fla.	4		443 Pa. 37	Fla.	4	
12	Ga.	12		444 N. Y. 44	Ga.	13	1
2	Idaho	2		445 Mich. 16	Idaho	2	
27	Ill.	27		446 Nebr. 6	Ill.	30	3
13	Ind.	12	—1	447 Kans. 8	Ind.	13	
11*	Iowa	10	—1	448 Calif. 15	Iowa	11	
8	Kans.	7	—1	449 Ill. 28	Kans.	8	
11	Ky.	10	—1	450 Ohio 25	Ky.	11	
8	La.	7	—1	451 Mo. 15	La.	8	
4	Maine	3	—1	452 Ind. 13	Maine	4	
6	Md.	6		453 Ark. 8	Md.	7	1
16	Mass.	16		454 N. J. 14	Mass.	18	2
13	Mich.	15	2	455 Mass. 17	Mich.	17	4
10	Minn.	10		456 N. Y. 45	Minn.	11	1
8	Miss.	7	—1	457 Pa. 38	Miss.	8	
16	Mo.	14	—2	458 Ga. 13	Mo.	16	
2	Mont.	2		459 Ky. 11	Mont.	3	1
6	Nebr.	5	—1	460 Iowa 11	Nebr.	6	
1	Nev.	1		461 Wis. 12	Nev.	1	
2	N. H.	2		462 N. Y. 46	N. H.	2	
12	N. J.	13	1	463 Ill. 29	N. J.	14	2
1*	N. M.	2	1	464 Tex. 21	N. M.	2	1
43	N. Y.	42	—1	465 Minn. 11	N. Y.	47	4
10	N. C.	10		466 Pa. 39	N. C.	12	2
3	N. D.	3		467 Ore. 4	N. D.	3	
22	Ohio	24	2	468 Ohio 26	Ohio	26	4
8	Okla.	8		469 W. Va. 7	Okla.	9	1
3	Ore.	3		470 S. C. 8	Ore.	4	1
36	Pa.	36		471 Ala. 11	Pa.	40	4
3	R. I.	3		472 Md. 7	R. I.	3	
7	S. C.	7		473 N. Y. 47	S. C.	8	1
3	S. D.	3		474 Tenn. 11	S. D.	3	
10	Tenn.	10		475 N. C. 12	Tenn.	11	1
18	Tex.	19	1	476 Mich. 17	Tex.	21	3
2	Utah	2		477 Maine 4	Utah	2	
2	Vt.	2		478 Calif. 16	Vt.	2	
10	Va.	9	—1	479 Mont. 3	Va.	11	1
5	Wash.	6	1	480 Pa. 40	Wash.	6	1
6	W. Va.	6		481 Mass. 18	W. Va.	7	1
11	Wis.	11		482 Va. 11	Wis.	12	1
1	Wyo.	1		483 Ill. 30	Wyo.	1	
				484 Mo. 16			

* If this method had been used in 1910, Iowa would have had 10 and New Mexico 2.

TABLE III

PEOPLE PER REPRESENTATIVE FOR HOUSE OF 435 MEMBERS

1920	No. of Reps.	Congressional District
U. S.	435	241,864
Ala.	10	234,817
Ariz.	1	309,754
Ark.	7	250,314
Calif.	14	244,716
Colo.	4	234,790
Conn.	6	230,105
Del.	1	223,003
Fla.	4	242,118
Ga.	12	241,319
Idaho	2	215,221
Ill.	27	240,196
Ind.	12	244,199
Iowa	10	240,402
Kans.	7	252,751
Ky.	10	241,663
La.	7	256,930
Maine	3	256,005
Md.	6	241,610
Mass.	16	240,772
Mich.	15	244,561
Minn.	10	238,566
Miss.	7	255,803
Mo.	14	243,147
Mont.	2	270,756
Nebr.	5	259,274
Nev.	1	75,820
N. H.	2	221,542
N. J.	13	242,762
N. M.	2	176,714
N. Y.	42	247,166
N. C.	10	255,912
N. D.	3	214,915
Ohio	24	239,975
Okla.	8	253,535
Ore.	3	261,130
Pa.	36	242,223
R. I.	3	201,466
S. C.	7	240,532
S. D.	3	210,413
Tenn.	10	233,789
Tex.	19	245,433
Utah	2	224,194
Vt.	2	176,214
Va.	9	256,576
Wash.	6	225,766
W. Va.	6	243,950
Wis.	11	239,210
Wyo.	1	193,487

rapidity of the method can be judged from the fact that these results can be recomputed (with the aid of Barlow's Tables of square roots and reciprocals and a calculating machine) in less than three hours.

The inequalities which remain between the several states, as indicated, for example, by the varying size of the congressional district,

TABLE IV
POPULATION DATA, 1920

State	Total population 1920	Indians not taxed	Population basis of apportionment
Ala.	2,348,174	.....	2,348,174
Ariz.	334,162	24,408	309,754
Ark.	1,752,204	.....	1,752,204
Calif.	3,426,861	830	3,426,031
Colo.	939,629	468	939,161
Conn.	1,380,631	.....	1,380,631
Del.	223,003	.....	223,003
Fla.	968,470	.....	968,470
Ga.	2,895,832	.....	2,895,832
Idaho	431,866	1,424	430,442
Ill.	6,485,280	.....	6,485,280
Ind.	2,930,390	.....	2,930,390
Iowa	2,404,021	.....	2,404,021
Kans.	1,769,257	.....	1,769,257
Ky.	2,416,630	.....	2,416,630
La.	1,798,509	.....	1,798,509
Maine	768,014	.....	768,014
Md.	1,449,661	.....	1,449,661
Mass.	3,852,356	.....	3,852,356
Mich.	3,668,412	.....	3,668,412
Minn.	2,387,125	1,469	2,385,656
Miss.	1,790,618	.....	1,790,618
Mo.	3,404,055	.....	3,404,055
Mont.	548,889	7,378	541,511
Nebr.	1,296,372	.....	1,296,372
Nev.	77,407	1,587	75,820
N. H.	443,083	.....	443,083
N. J.	3,155,900	.....	3,155,900
N. M.	360,350	6,922	353,428
N. Y.	10,385,227	4,240	10,380,987
N. C.	2,559,123	.....	2,559,123
N. D.	646,872	2,126	644,746
Ohio	5,759,394	.....	5,759,394
Okla.	2,028,283	.....	2,028,283
Ore.	783,389	.....	783,389
Pa.	8,720,017	.....	8,720,017
R. I.	604,397	.....	604,397
S. C.	1,683,724	.....	1,683,724
S. D.	636,547	5,308	631,239
Tenn.	2,337,885	.....	2,337,885
Tex.	4,663,228	.....	4,663,228
Utah	449,396	1,008	448,388
Vt.	352,428	.....	352,428
Va.	2,309,187	.....	2,309,187
Wash.	1,356,621	2,025	1,354,596
W. Va.	1,463,701	.....	1,463,701
Wis.	2,632,067	762	2,631,305
Wyo.	194,402	915	193,487
Total	105,273,049	60,870	105,212,179
D. C.	437,571	.....	.....
Total	105,710,620	.....	.....

TABLE V

DETAILS OF THE COMPUTATION REQUIRED IN THE APPLICATION OF THE PROPOSED METHOD TO THE 1920 APPORTIONMENT

Condensed Priority List for Indices Above 244,500			Complete Priority List for Indices Between 244,500 and 219,000			
1920			436	N. Y.	43	244,275
Ariz.	1	.....	437	N. C.	11	244,003
Del.	1	.....	438	Va.	10	243,410
Wyo.	1	.....	439	La.	8	240,336
Nev.	1	.....	440	Miss.	8	239,281
Mont.	2	382,906	441	Tex.	20	239,219
Ore.	3	319,817	442	Okla.	9	239,035
Utah	2	317,058	443	Pa.	37	238,927
Maine	3	313,540	444	N. Y.	44	238,659
N. H.	2	313,307	445	Mich.	16	236,795
Idaho	2	304,368	446	Nebr.	6	236,684
Nebr.	5	289,878	447	Kans.	8	236,427
Fla.	4	279,573	448	Calif.	15	236,419
La.	7	277,516	449	Ill.	28	235,867
Miss.	7	276,298	450	Ohio	25	235,126
Kans.	7	273,002	451	Mo.	15	234,902
Va.	9	272,140	452	Ind.	13	234,619
Colo.	4	271,112	453	Ark.	8	234,148
Okla.	8	271,041	454	N. J.	14	233,931
Ark.	7	270,371	455	Mass.	17	233,583
N. C.	10	269,755	456	N. Y.	45	233,295
W. Va.	6	267,244	457	Pa.	38	232,554
Md.	6	264,671	458	Ga.	13	231,852
N. D.	3	263,216	459	Ky.	11	230,417
S. C.	7	259,804	460	Iowa	11	229,214
S. D.	3	257,702	461	Wis.	12	229,026
Ind.	12	255,058	462	N. Y.	46	228,167
Ky.	10	254,735	463	Ill.	29	227,589
Calif.	14	253,954	464	Tex.	21	227,542
Iowa	10	253,406	465	Minn.	11	227,463
Mich.	15	253,144	466	Pa.	39	226,513
N. J.	13	252,674	467	Ore.	4	226,145
Mo.	14	252,325	468	Ohio	26	225,902
Tex.	19	252,156	469	W. Va.	7	225,854
Conn.	6	252,068	470	S. C.	8	224,997
Ga.	12	252,050	471	Ala.	11	223,890
Minn.	10	251,470	472	Md.	7	223,688
Wis.	11	250,885	473	N. Y.	47	223,260
N. Y.	42	250,162	474	Tenn.	11	222,909
N. M.	2	249,911	475	N. C.	12	222,743
Vt.	2	249,204	476	Mich.	17	222,430
Mass.	16	248,669	477	Maine	4	221,707
Ala.	10	247,519	478	Calif.	16	221,149
Wash.	6	247,314	479	Mont.	3	221,071
R. I.	3	246,744	480	Pa.	40	220,778
Tenn.	10	246,435	481	Mass.	18	220,225
Pa.	36	245,659	482	Va.	11	220,172
Ohio	24	245,136	483	Ill.	30	219,872
Ill.	27	244,771	484	Mo.	16	219,731
			485	Ariz.	2	219,029

are unavoidable inequalities. Any attempt to reduce the inequality between any two states by transferring a representative from one state to the other will be found to result in increasing that inequality instead of reducing it.

VI. A MEASURE OF TOTAL OR AVERAGE ERROR

The method of equal proportions, as developed above, provides a complete solution of the problem of finding the *best* apportionment for any given values of $A, B, C, \ldots$ and N; but it does not provide any measure of comparison between two apportionments which are less good than the best; nor any measure of comparison between two apportionments involving different values of N.

Although such a measure of error is of little or no importance in practice, it may be of interest to note that a satisfactory measure, E, can be supplied as follows:*

Let a be the actual and $\alpha = (A/P)N$ the theoretical number of representatives assigned to a typical state A. Then

$$e = \frac{(a/A)-(\alpha/A)}{\sqrt{(a/A)(\alpha/A)}} = \frac{(A/\alpha)-(A/a)}{\sqrt{(A/\alpha)(A/a)}} = \frac{a-\alpha}{\sqrt{a\alpha}}$$

may be regarded as expressing the *error affecting an individual citizen* of that state. Taking the sum of the squares of all such errors for all the inhabitants of the country, we have as the *total error*

$$T = A\frac{(a-\alpha)^2}{a\alpha} + B\frac{(b-\beta)^2}{b\beta} + \ldots;$$

whence, dividing by the total population, P, and taking the square root, we have

$$E = \sqrt{T/P}$$

as the *average error* of the whole apportionment. For the purpose of computation, it is convenient to write E in the form:

$$E = \sqrt{T'/N}, \text{ where } T' = (a-\alpha)^2/a + (b-\beta)^2/b + \ldots$$

It can be shown that in any given case the method which makes the value of this total or average error a minimum, is precisely the method of equal proportions.

* The expression E for the average error is new, and is to be presented to the American Mathematical Society on April 23, 1921. An earlier but less satisfactory expression was proposed by Professor F. W. Owens on February 26, 1921 (see Appendix II).

APPENDIX I

CRITICISM OF FOUR ALTERNATIVE METHODS BASED ON A WRONG INTERPRETATION OF "NEARLY EQUAL"

If the inequality between two quantities were to be measured by their *absolute difference*, instead of by their percentage difference, the four parts of our fundamental principle would lead to four distinct and conflicting methods of apportionment, which may be called Methods I*a*, I*b*, I*c*, I*d*. Since there is no mathematical ground for choosing between I*a* and I*b*, or between I*c* and I*d*, the result would be a hopeless dilemma, which shows conclusively that the "absolute difference" interpretation is not the interpretation that is appropriate to the present problem.

In contrast with these four conflicting methods, the method of equal proportions, which may be called Method I, involves no such dilemma, since it satisfies all four of the requirements of the fundamental principle simultaneously.

The following notes on Methods I*a*, I*b*, I*c*, I*d* may be of interest.

Method I*a* leads to a "working rule" with the following "multipliers":

$$\frac{1+2}{2(1\times 2)},\quad \frac{2+3}{2(2\times 3)},\quad \frac{3+4}{2(3\times 4)},\quad \frac{4+5}{2(4\times 5)},\quad \ldots$$

and may therefore be called the *method of the harmonic mean*. This method, whenever it differs from Method I, favors the small states more than Method I does. (See Example 1*a* and Example 1.)

Method I*b*, taken in connection with the requirement that every state shall have at least one representative, leads to a "working rule" with the following "multipliers":

$$2/3,\quad 2/5,\quad 2/7,\quad 2/9,\quad \ldots$$

and may therefore be called the *method of the arithmetic mean*. This method, whenever it differs from Method I, favors the large states more than Method I does. (See Example 1*b* and Example 1.)*

The amount by which the results of Methods I*a* and I*b* may diverge is often large, as illustrated in Examples 2, 2*a*, and 2*b*.

Methods I*c* and I*d* also determine two distinct methods (for which no "working rules" have been devised). It can be shown that I*c* favors the small states even more than I*a* does, while I*d* favors the

* The working rule for Method I*b* was first employed by Professor W. F. Willcox in 1910, as the basis of his "method of major fractions" (see Appendix II). The existence of the correlative Method I*a* was not known at that time.

large states even more than I*b* does. (See Examples 1*c* and 1*d*, and Example 1.)

All four of these conflicting methods should therefore be rejected in favor of Method I, which satisfies all four parts of our fundamental principle simultaneously.

APPENDIX II

CRITICISM OF OTHER METHODS OF APPORTIONMENT*

1. *The Vinton Method of 1850.* In this method, long in use in Congress, the rule of procedure is as follows:

First, compute the exact quota for each state, putting aside for separate consideration those states whose quotas are less than one. Next,

Example 1a

	Ia	I Ib
A 454	4	5
B 246	3	2
700	7	7

Example 1b

	I Ia	Ib
A 451	4	5
B 249	3	2
700	7	7

Example 1c

	Ic	I, Ia Ib, Id
A 564	5	6
B 136	2	1
700	7	7

Example 1d

	I, Ia Ib, Ic	Id
A 548	5	6
B 152	2	1
700	7	7

Example 1

	Ic	Ia	I	Ib	Id
A 564	5	6	6	6	6
B 548	5	5	5	5	6
C 454	4	4	5	5	5
D 451	4	4	4	5	5
E 249	3	3	3	2	2
F 246	3	3	2	2	2
G 152	2	2	2	2	1
H 136	2	1	1	1	1
2800	28	28	28	28	28

Example 2a

	Ia	I	Ib
A 9420	92	93	96
B 147	2	2	1
C 146	2	2	1
D 144	2	2	1
E 143	2	1	1
10000	100	100	100

Example 2

	Ia	I	Ib
A 9430	92	94	96
B 144	2	2	1
C 143	2	2	1
D 142	2	1	1
E 141	2	1	1
10000	100	100	100

Example 2b

	Ia	I	Ib
A 9440	92	95	96
B 143	2	2	1
C 140	2	1	1
D 139	2	1	1
E 138	2	1	1
10000	100	100	100

assign to each of the remaining states a number of representatives equal to the largest whole number contained in the exact quota for that state, and add the results. Then ascertain, by subtraction, how many additional representatives must be assigned "for fractions." Finally, as-

* On the Vinton, Hill, and Willcox methods, see papers by J. A. Hill and W. F. Willcox in *House of Representatives Report No. 12*, pp. 1–108, of the Sixty-second Congress, First Session, April 25, 1911; also the presidential address of W. F. Willcox, read at the annual meeting of the American Economic Association, December, 1915, and published in the *American Economic Review*, vol. 6, no. 1, Supplement, pp. 1–16, March, 1916. Tables for the 1920 apportionment according to the Willcox method are printed in *House of Representatives Report* No. 1173, pp. 1–28, of the Sixty-Sixth Congress, Third Session, January 8, 1921. On the d'Hondt method, see John H. Humphreys, *Proportional Representation*, London, 1911, pp. 178–180 and 188–190. On the Owens theory of the Willcox method, nothing has yet been published (April 1, 1921).

sign these additional representatives to as many states as may be necessary, taking the states, for this purpose, in the order of magnitude of the fractions themselves (that is, the state with the largest fraction is the first to receive an additional representative).

This method sometimes leads to absurd results, as shown in Example 3. Here when the total number of representatives is increased from 100 to 101, the representation of one of the states actually decreases from 11 to 10. This paradox first came to the attention of Congress in the tables prepared in 1881, which gave Alabama 8 members when $N=299$ and only 7 members when $N=300$; it is therefore known as the *Alabama Paradox*. (See Willcox, *loc. cit.*, p. 10.) Any method which is capable of producing such a result as this is clearly unsatisfactory.

2. *The Hill Method of alternate ratios.* This method, proposed by Dr. Joseph A. Hill in 1910, proceeds as in the Vinton method, except that for the purpose of assigning the additional representatives "for fractions" the states are arranged in order of magnitude of the quantity $A/\sqrt{x(x+1)}$, where x is the number already assigned to state A (that is, the largest whole number in the true quota for that state) and $x+1$ is the next larger number.

As already stated in a foregoing footnote, this method is the best of all hitherto known methods, Dr. Hill's researches of 1910 containing the first discussion of the problem which was based on the correct principle of percentage differences. In fact, if Dr. Hill had dropped the arbitrary requirement that the best assignment to any state must be either x or $x+1$, he would have been led immediately to the method of equal proportions. As it stands, his method of alternate ratios might some time produce an Alabama Paradox, as is shown by Example 4.

3. *The Willcox Method of major fractions*, devised by Professor W. F. Willcox of Cornell University in 1910, was used in the 1910 apportionment, and again in the bill which passed the House on January 19, 1921.

In this method, the population of each state is divided by as many of the numbers $1\frac{1}{2}$, $2\frac{1}{2}$, $3\frac{1}{2}$, . . . as may be necessary, and all the results are then arranged in a priority list in order of magnitude. The representatives, beginning with the 49th, are then assigned to the several states in the order in which the names of the states occur in this priority list.

This method (though arrived at by quite different reasoning involving the use of a "variable ratio" or divisor) is equivalent in its results to Method I*b* (the method of the arithmetic mean, taken in connection with the requirement that every state shall have at least one represen-

tative), and is therefore open to all the objections that have already been raised against that method. It satisfies part I*b* of our fundamental principle only on the assumption that absolute differences are more significant than percentage differences, which is, to say the least, a questionable assumption; and it fails altogether to satisfy parts I*a*, I*c*, and I*d*, no matter which interpretation of "nearly equal" is adopted. (See Example 1.)

It may be added that the name "method of major fractions" is somewhat misleading. *A method which would assign an additional representative for every major fraction in the true quota would indeed have much to recommend it; but no such method is possible.* The Willcox "major fraction" is not a major fraction of the true quota, but a major fraction of an artificial quota, scaled up or down from the true quota (by means of a sliding divisor) in order to meet the requirements of the particular method of computation.

Example 3. Vinton Method. (Paradox)

	N=100		N=101	
	Quota		*Quota*	
A 453	*45.3*	45	*45.753*	46
B 442	*44.2*	44	*44.642*	45
C 105	*10.5*	11	*10.605*	10
1000	*100.0*	100	*101.000*	101

Example 4. Hill Method. (Paradox)

	N=100		N=101	
	Quota		*Quota*	
A 154,550	*30.91*	30	*31.2191*	31
B 154,500	*30.90*	30	*31.2090*	31
C 154,450	*30.89*	30	*31.1989*	31
D 7,400	*1.48*	2	*1.4948*	2
E 7,350	*1.47*	2	*1.4847*	2
F 7,300	*1.46*	2	*1.4746*	2
G 7,250	*1.45*	2	*1.4645*	1
H 7,200	*1.44*	2	*1.4544*	1
500,000	*100.00*	100	*101.0000*	101

Example 5.

	d'Hondt	I, Ia, Ib, Ic, Id
A 9802	99	98
B 198	1	2
10000	100	100

For example, in the 1920 apportionment for 435 representatives, the "ratio" or divisor used in the Willcox method, in order to produce the desired number of major fractions, is 1 to 242,415; whereas the true ratio, in the sense in which the phrase "1 to 30,000" is used in the Constitution, is 1 to 241,867.

4. *The d'Hondt method*, devised by Professor Victor d'Hondt of the University of Ghent, Belgium, is now widely used in European elections.

In this method, the population of each state (or political party) is divided by as many of the numbers 1, 2, 3, 4, . . . as may be necessary, and all the results are then arranged in a priority list in order of magnitude. The representatives, beginning with the first, are then assigned to the several states in the order in which the names of the states occur in this priority list.

In this plan, some states may receive several representatives before other states receive any, so that in order to meet the United States requirement that every state shall have at least one representative, the rule would have to be modified by changing "beginning with the first" to "beginning with the 49th." With or without this modification, the method favors the larger states more than does any other known method, as illustrated in Example 5.

This method therefore violates all four parts of our fundamental principle, no matter which interpretation of "nearly equal" is adopted.

5. *The Owens method of least error* (so-called), which was proposed by Professor F. W. Owens of Cornell University at a recent meeting of the American Mathematical Society, February 26, 1921, is not a new method of apportionment, but a new mathematical basis for the Willcox method of major fractions.

Professor Owens, having in mind the theory of Least Squares, starts with a selected expression for the "total error" of an apportionment, namely:

$$Q=A[(a/A)-k]^2+B[(b/B)-k]^2+C[(c/C)-k]^2+ \quad . \quad . \quad . \quad ,$$

and then shows that the method which minimizes this total error Q is precisely the Willcox method.

Here k denotes the true value of a/A or b/B or c/C, . . . It may be noted, however, that any other value of k, even $k=0$, might be substituted without affecting the final result.

In the opinion of the present writer, there is no sufficient reason for selecting the fraction a/A rather than the fraction A/a, or for selecting the absolute difference rather than the percentage difference, in building up the expression for Q. If one must use the idea of total error (which is not required in the method of equal proportions), a better expression for it would be the value T given in a previous paragraph.

In any case, the failure of the Willcox method to meet most of the requirements of our fundamental principle is not in any way affected by the Owens theory. That fundamental principle is an inevitable consequence of the plain provisions of the Constitution, with nothing read between the lines; and the only method which can satisfy the whole of that principle is the method of equal proportions.

EFFECT ON THE RESERVE RATIO OF CHANGES IN RESERVES AND IN LIABILITIES

By E. A. Goldenweiser, *Federal Reserve Board*

With the growing importance of the reserve ratio of Federal Reserve banks as an indicator of the credit condition of the country, it has become of interest to study in detail the various elements that have an effect on that ratio. In this paper no reference is made to the economic and financial conditions affecting the reserve position of the Federal Reserve banks, but an effort is made to determine the effect on the reserve ratio of changes in reserves and in liabilities.

In order to make this discussion understandable to persons not familiar with Federal Reserve banking, it may be stated that the reserve ratio of the Federal Reserve banks is calculated by dividing total cash reserves by combined liabilities on deposits and on Federal Reserve notes. The formula is as follows: Reserve ratio $= \frac{\text{Cash reserves}}{\text{Deposits} + \text{Notes}}$. It is clear, therefore, that the reserve ratio increases as cash reserves increase, and decreases as liabilities increase, and vice versa.

The problem was brought prominently to the attention of the Federal Reserve Board by the large increase in reserves in recent months (owing to the inward gold movement) accompanied by continuous declines in liabilities. The reserve ratio stood at the end of May at above 57 per cent, as compared with a low ratio of 42 per cent about a year ago. It has been a matter of interest and of importance to determine to what extent this increase in the reserve ratio has been caused by additions to reserves, and to what extent by reductions of liabilities.

One way of studying the matter is by a comparison of two given dates, and another way is by an analysis of continuous changes for a given period. Each of these methods lends itself to study in a rough approximate way, but each can also be approached with a view to reducing it to an exact mathematical formula or index. The various methods that have been followed will be briefly described in the discussion below.

COMPARISON FOR TWO GIVEN DATES

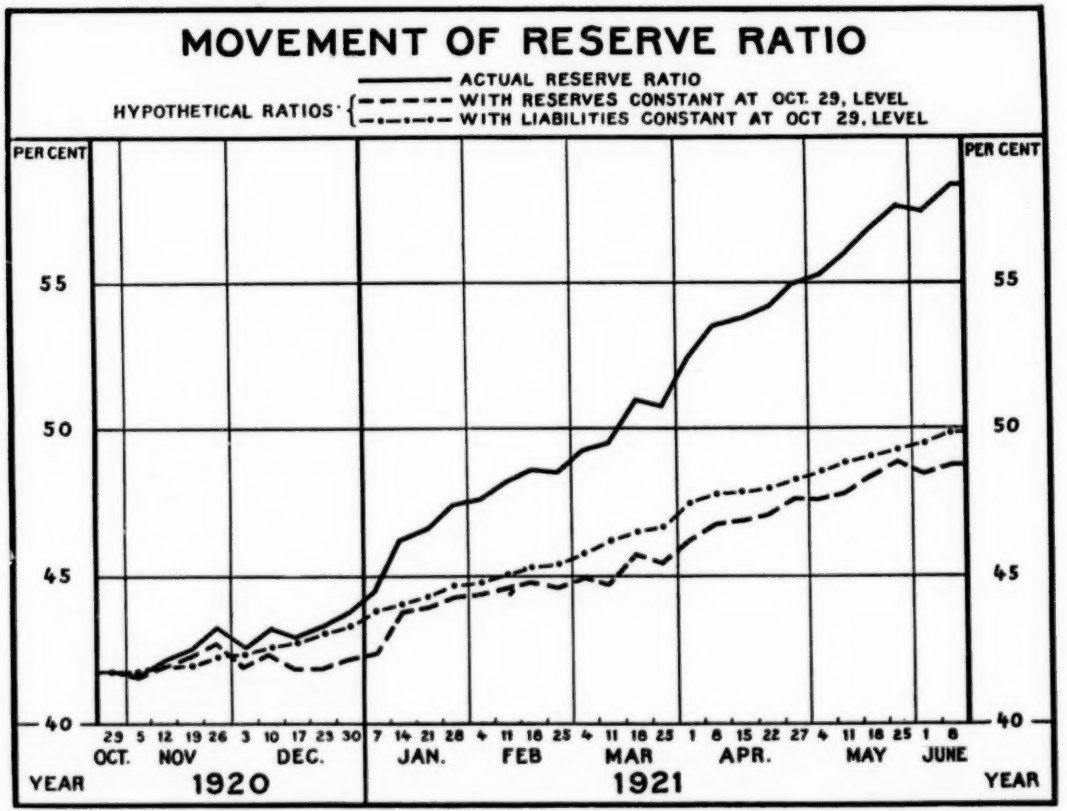
Selecting for the purpose of this discussion the end of October, 1920, and the end of May, 1921, as the boundaries of the period to be studied,

we find the pertinent facts to have been as follows: At the end of October, 1920, the banks' cash reserves were $2,168,038,000, total deposits, $1,845,722,000, and Federal Reserve note circulation, $3,351,303,000, resulting in a reserve percentage of 41.7. By June 1, 1921, these figures had changed in the following manner: Reserves had increased by $400,787,000, or 18.5 per cent, deposits had declined by $125,332,000, and notes, by $600,004,000, the aggregate decline in liabilities being $725,336,000, or 13.96 per cent. The reserve ratio on the latter date was 57.4 per cent. A rough way of estimating the effect of the two factors, or of the three factors, on the reserve ratio is by assuming consecutively that each of the factors had remained unchanged, and comparing the resultant reserve ratios with the actual reserve ratio. In the present case it would work out as follows: If reserves had remained stationary at the end-of-October figure of $2,168,038,000, the reserve ratio on June 1 would have been 48.5 per cent. If note circulation had remained stationary, the percentage would have been 50.6 per cent; if deposit liabilities had remained unchanged, it would have been 55.9 per cent; if no change had taken place in either class of liabilities, or if the changes had been mutually compensatory, the ratio would have been 49.4 per cent. The situation can be summarized as follows:

	Per cent
Actual ratio on June 1, 1921	57.4
Hypothetical ratio, with the specified items unchanged at October 29, 1920, level:	
Reserves	48.5
Total liabilities	49.4
Deposits	55.9
Notes	50.6

From these figures it appears that changes in reserves were the largest factor in the increase of the reserve ratio between October 29 and June 1, as the retention of the reserves at their October level would have caused the greatest departure from the ratio actually shown for June 1. But reduction in liabilities was nearly as powerful a factor, while of the two kinds of liabilities, notes in circulation affected the ratio a great deal more than deposits, which have changed comparatively little since October.

This method gives a general impression of the changes that took place between the two terminal dates, but an effort was made also to obtain a formula that would distribute the increase in the reserve ratio between the factors involved with mathematical accuracy. Such an algebraic formula was worked out by Mr. J. R. Van Fossen of the Board's staff. He assumed that A equals the percentage increase in reserves and B the percentage increase in combined Federal Reserve


MOVEMENT OF RESERVE RATIO
ACTUAL RESERVE RATIO
HYPOTHETICAL RATIOS
WITH RESERVES CONSTANT AT OCT. 29, LEVEL
WITH LIABILITIES CONSTANT AT OCT 29, LEVEL
PER CENT
55
50
45
40
29 5 12 19 26 3 10 17 23 30 7 14 21 28 4 11 18 25 4 11 18 25 1 8 15 22 27 4 11 18 25 1 8
OCT. NOV DEC. JAN. FEB MAR APR. MAY JUNE
YEAR
1920
1921
YEAR

note and deposit liabilities. Since the reserve ratio varies directly as reserves and inversely as combined note and deposit liabilities, A will represent also the percentage increase in the reserve ratio, when liabilities remain constant, while $\frac{1}{1+B}-1$ will represent the percentage increase in reserve ratio when reserves remain constant and liabilities alone vary. When both reserves and combined liability vary, the percentage change in the resulting reserve ratio will be caused by the two factors in the ratio of A to $\left(\frac{1}{1+B}-1\right)$ or $\left(\frac{-B}{1+B}\right)$. Accordingly, the effect attributable to the change in reserves is given by the expression $\frac{A}{A+\left(\frac{-B}{1+B}\right)}$ times the percentage increase in the reserve ratio, while that of the liabilities is given by the expression $\frac{\frac{-B}{1+B}}{A+\left(\frac{-B}{1+B}\right)}$ times the percentage increase in the reserve ratio (R) Simplifying these expressions we obtain $R\left(\frac{A+AB}{A+AB-B}\right)$ and $R\left(\frac{-B}{A+AB-B}\right)$.

This formula works out satisfactorily the apportionment of changes in the reserve ratio as long as both of the factors affect the ratio in the same direction, *i. e.*, as long as they move in opposite directions. When, however, reserves and liabilities both increase or both decrease, the formula gives illogical results because of technical mathematical reasons having to do with apportionment of a quantity in proportion to negative and positive factors and with the disturbing appearance of zero in the equation at the point where the two opposite changes exactly offset each other. For this reason it is not possible to use this formula in cases in which reserves have increased and liabilities have also increased. To meet this situation, which apparently does not lend itself to accurate mathematical determination, a method of approximation has been devised. To take a concrete though purely hypothetical example: Suppose that cash reserves equaled 100 and liabilities 200, then the ratio would be $\frac{100}{200}$, or 50 per cent. Suppose that reserves increased by 50 per cent to 150 and liabilities by 12½ per cent to 225, then the ratio would be $\frac{150}{225}=66\frac{2}{3}$ per cent, an increase in ratio from 50 to 66⅔ per cent, or (on the basis of 50 as 100) an increase of 33⅓ per

cent. The problem is: What are the two factors, one positive and one negative, that have resulted in the 33⅓ per cent increase in reserves? The 50 per cent increase in reserves, with liabilities constant, would have caused a 50 per cent increase in ratio; the 12.5 per cent increase in liabilities, with reserves constant, would have caused a 11.3 per cent decrease in the ratio; and the result of the two factors operating independently would have been an increase of 50 per cent and a decrease of 11 per cent, giving an ultimate increase of 39 per cent. But the actual increase was only 33 per cent. Distribute the difference $(39-33=6)$ in proportion to the "motive power" of the changes in reserves and in liabilities, namely 50 and 11, or roughly 5 to 1. The increase due to the reserves will thus not be 50 per cent, but only 45 per cent, and the decrease due to increase in liabilities will not be 11 per cent, but 12 per cent. Answer: the increase of 33 per cent in ratio is attributable roughly to an increase of 45 per cent due to the increase in reserves, offset in part by a decrease of 12 per cent caused by the increase in liabilities. This method, although not absolutely accurate, affords a sufficiently close approximation to accuracy to answer all practical requirements.

The apportionment between notes and deposits of the increase or decrease in ratio attributable to the change in liabilities is a simple problem in proportion. The division must be in proportion to the effect that the change in each factor has had on the combined liabilities. Further details on this simple point do not appear to be necessary in this discussion.

CONTINUOUS COMPARISONS FOR A PERIOD OF TIME

As against the above methods of comparing two separate dates, there have been made also studies of the week-to-week relationship between the changes in reserves and in liabilities. Here again a crude general study may be made by assuming that each factor in turn remained constant, and in seeing how the result in the reserve ratio would vary from the actual reserve ratio. The accompanying chart is based on figures computed on these assumptions. It shows that on the whole changes in reserves and in liabilities have been of approximately equal importance in causing changes in the reserve ratio during the period under discussion, although the changes in reserves have been slightly more potent than changes in liabilities, as evidenced by the fact that the curve showing the ratio with reserves constant is almost continuously lower than the curve showing the ratio with liabilities constant.

However, a more accurate way of determining these relationships was thought desirable, and a coefficient of correlation was worked out. But the results were unsatisfactory, mainly because the coefficient of

correlation in its derivation assumes a norm or base and works from that base. The Pearson coefficient originated in biological study where a norm is assumed to exist. In banking statistics during a period of rapid change without much historic precedent, there is no norm, and a correlation worked out in relation to an arbitrary amount for a given date or to a short-time average is not a satisfactory method of procedure. For this reason it was decided to compare the changes from week to week, using each successive week as a base for the change for the following week. In this manner an index of divergence was worked out. In developing the index, the successive steps may be described as follows:

1. The actual amounts of each of the five items involved (reserves, notes, deposits, combined liabilities, and ratio) were listed in millions of dollars for each week from October 29, 1920, to June 1, 1921.

2. For each of the items, the increases or decreases from one week to the next in absolute amounts were written in a second column.

3. The percentages of increase or decrease for each week were calculated.

4. For each set of quantities the divergence between the percentage of change from week to week was figured.

5. These divergences were added and then divided by the number of weeks; the quotient is "the index of divergence."

Of the various steps in the procedure, only the fourth calls for discussion, the others being plain arithmetical propositions.

In the following table the absolute amounts are omitted in order to save space and also because they have no bearing on the method; only the percentages and the divergences are shown. In order to understand the method, it should be borne in mind that what is done is to compare the four sets of relationships:

(*a*) Between changes in the reserve ratio and in cash reserves.

(*b*) Between changes in the reserve ratio and in deposit and note liabilities combined.

(*c*) Between changes in the reserve ratio and in Federal Reserve notes.

(*d*) Between changes in the reserve ratio and in net deposits.

To begin with the first relationship:

During the week ending November 5, cash reserves increased by 0.08 per cent. During the same week, the reserve ratio decreased by 0.10 per cent. The relationship between cash reserves and the reserve ratio being direct, a decline in cash reserves—other things being equal—would result in a proportionate decline in the ratio. The divergence, therefore, equals the sum of the percentage of increase in

Week ending—	Percentage of increase (+) or of decrease (−) in					Divergence in percentage changes in reserve ratio and—			
	Cash reserves	Federal Reserve notes	Total deposits	Notes and deposits combined	Reserve ratio	Cash reserves	Federal Reserve notes	Total deposits	Notes and deposits combined
1920									
October 29	+ .50	− .15	+1.59	+ .47	+ .02	.48	.13	1.61	.49
November 5	+ .08	+ .09	+ .32	+ .17	− .10	.18	.01	.22	.07
12	+ .47	− .75	− .33	− .60	+1.06	.59	.31	.73	.46
19	+ .01	− .65	−1.36	− .90	+ .95	.94	.30	.41	.05
26	+ .69	+ .55	−3.83	−1.01	+1.72	1.03	2.27	2.11	.71
December 3	+ .13	− .41	+5.69	+1.70	−1.55	1.68	1.96	4.14	.15
10	+ .65	− .01	−2.07	− .75	+1.41	.76	1.40	.66	.66
17	+ .45	+ .98	+1.03	+1.00	− .53	.98	.45	.50	.47
23	+ .64	+1.81	−3.24	+ .02	+ .61	.03	2.42	2.63	.63
30	+ .55	−1.77	+1.56	− .63	+1.20	.65	.57	2.76	.57
1921									
January 7	+1.23	−2.23	+2.67	− .52	+1.76	.53	.47	4.43	1.24
14	+ .51	−3.38	−2.93	−3.22	+3.84	3.33	.46	.91	.62
21	+ .55	−1.40	+1.69	− .28	+ .84	.29	.56	2.53	.56
28	+ .81	− .79	− .82	− .80	+1.63	.82	.84	.81	.83
February 4	+ .27	− .49	+ .52	− .12	+ .38	.11	.11	.90	.26
11	+ .69	− .82	− .16	− .58	+1.26	.57	.44	1.10	.68
18	+ .46	− .43	− .42	− .42	+ .89	.43	.46	.47	.47
25	+ .17	+ .47	+ .12	+ .34	− .15	.32	.32	.03	.19
March 4	+ .80	− .30	−1.26	− .66	+1.46	.66	1.16	.20	.80
11	+ .93	−1.21	+3.21	+ .43	+ .51	.42	.70	3.72	.94
18	+ .70	−1.43	−3.75	−2.31	+3.07	2.37	1.64	.68	.76
25	+ .30	−1.09	+3.73	+ .72	− .41	.71	1.50	3.32	.31
April 1	+1.62	− .77	−2.81	−1.56	+3.23	1.61	2.46	.42	1.67
8	+ .84	− .49	−2.45	−1.24	+2.10	1.26	1.61	.35	.86
15	+ .13	− .88	+ .55	− .34	+ .47	.34	.41	1.02	.13
22	+ .31	− .41	− .32	− .38	+ .69	.38	.28	.37	.31
27	+ .48	− .93	− .13	−1.09	+1.59	1.11	.66	1.46	.50
May 4	+ .60	− .05	+ .18	+ .03	+ .56	.04	.51	.74	.59
11	+ .71	− .84	+ .26	− .42	+1.14	.43	.30	1.40	.72
18	+ .45	−1.34	− .97	−1.20	+1.66	1.21	.32	.69	.46
25	+ .36	−1.18	− .62	− .97	+1.34	.98	.16	.72	.37
June 1	+ .41	+ .60	+ .85	+ .70	− .28	.69	.32	.57	.42
Total divergence						25.93	25.51	42.61	17.95
Index of divergence						.81	.80	1.33	.56

cash reserves and the percentage of decrease in the reserve ratio, that is, 0.08+0.10=0.18. The same calculation is made for each week of the period. These divergences are then added, totaling 25.45, which, when divided by the number of weeks, gives an index of divergence of 0.82.

In measuring the relationship between the reserve ratio and the liabilities, either separately or combined, the method of procedure is modified by the fact that the relationship is inverse. For example, taking the same week as above, Federal Reserve notes and deposits combined show an increase of 0.17 per cent. During the same week, the reserve ratio shows a decrease of 0.10 per cent. The difference between these declines is 0.07. The same calculation for each week of the period, when averaged, gives an index of divergence of 0.56.

The method used in the calculation of the index of divergence of Federal Reserve notes and total deposits is exactly the same as that used in the calculation of the two combined. The indices of divergence are 0.80 for Federal Reserve notes and 1.33 for total deposits. In view of the fact that Federal Reserve notes and total deposits are each only part of the divisor in calculating the ratio, it is to be expected that the divergence would be greater in the case of those partial factors than in the case of total cash reserves or of total liabilities. It will be noted, however, that the index of divergence for Federal Reserve notes is no larger than that for cash reserves. This means that in spite of the handicap of being only a partial factor, Federal Reserve notes have, as a matter of fact, fluctuated in as close harmony with the reserve ratio as have cash reserves. On the other hand, total deposits show an index of divergence that is much greater than that of any of the other factors involved.

It is admitted that this method is crude. A much more refined way of arriving at the result has been worked out by G. P. Watkins in a paper to be published later. Watkins works with logarithms instead of percentages, because logarithms can be added, subtracted, and divided without damage to their correctness, whereas percentages are not adapted to that use for accurate purposes for the reason that an increase of 10 per cent does not correspond to a decrease of 10 per cent, but to one of 9 per cent. Watkins, who attacked the problem in connection with a study of grain prices, also makes corrections for variability and trend, and his results are of a high order of mathematical accuracy. For the purposes of the Federal Reserve Board, however, it is probable that the index of divergence, as described in this paper, is sufficiently accurate to indicate the relative closeness of variations in the reserve ratio and in reserves, on the one hand, and in notes and deposits on the other.

ELECTRICAL STATISTICS AS A BAROMETER OF INDUSTRIAL ACTIVITY

By Robert M. Davis, *Statistical Editor, Electrical World*

Grimm's tale of the boy who possessed the seven-league boots is fully exemplified in the phenomenal growth of the electric light and power industry, which has developed by leaps and bounds and has become one of the fundamental elements of America's industrial fabric. There is probably no primary industry that can show such rapid statistical growth. Certainly none can show a more enthusiastic adoption by the American people, both in the home and in the factory.

During the past six years the yearly output of the central generating stations of the country has increased from 16,782,000,000 kilowatt-hours in 1914 to 45,678,000,000 kilowatt-hours in 1920, or 173 per cent. During the same period the annual gross revenue increased from $336,980,000 to $882,750,000, or 162 per cent. The installed rating of the central generating plants has increased from 7,000,000 kilowatts in 1914 to 13,750,000 kilowatts on January 1, 1921, and during the same period the capital invested in the light and power industry has increased from $2,706,000,000 to $4,050,000,000.

These figures point to several pertinent conclusions, but above all they show clearly that electrical energy has fused itself into the very heart of American life; that it is no longer an experiment or a luxury, but a power controlling, and so reflecting, both the domestic and the industrial life of our country. In a section of the country that today has poor electric facilities one unfailingly finds a community practically without industrial life, except possibly agriculture or mining; and as a rule the inhabitants of that region are either nomads or are living far below the average scale to be found in the remaining portions of the United States.

The production of electrical energy by the central generating plants of the country depends upon two separate and distinct elements, the domestic and municipal lighting load, and the industrial or manufacturing load. The lighting load varies with the season, increasing materially in the fall and winter and decreasing as the winter wanes and the long summer days approach. The industrial demand for electrical energy, on the other hand, depends solely upon the rate of operations in the various industries. A slack season during which only a portion of the machinery is placed in operation results in a call for less

electrical energy for power purposes, while a season of intense industrial activity may result in a power demand far in excess of the power companies' capacity to fulfil. Every American industry has now adopted electrical energy as a source of power to such an extent that the least depression in any primary industry in any section of the country is at once reflected in the operation of the central generating plants that furnish electrical energy to that section. As time passes and industry is more and more electrified, the reports of central station operations will more fully reflect industrial activity. But to the trained statistician, ready to read between the lines and to notice the least variance in the operation of the light and power industry, it is now possible to interpret central station monthly returns in such detail that with a general knowledge of industrial affairs the extent of productive curtailment in any industry can be readily ascertained. Such interpretation may be made for a certain city or community, based upon the detail returns of one or more of the electric light and power companies serving that community. Or industrial conditions in a large section of the country may be clearly defined by a study of group operation figures from a large percentage of the light and power companies of that section. And, finally, the general tone of industrial America is pictured in an unmistakable manner by the tabulation of the monthly operations of a large percentage of the light and power companies of the country.

Let us study briefly the output figures of the central generating and distributing companies of the country for the month of March, 1921, as received by the Statistical Department of the *Electrical World*. Reports are received from companies representing 78 per cent of the installed rating of the central generating plants of the country; the 22 per cent not making returns are small plants located for the most part in purely agricultural regions. Reports are received monthly for electrical output, gross revenue, and operating and maintenance expenses. The story which the gross revenue and operating expenses tells is a very important one to the light and power industry, but it has little bearing on the subject under consideration.

Table I gives the aggregate central station returns for the twelve month period ending with March, 1921. Table II gives similar figures for the same period, but by the five sections of the country. The diagram shows the electric light and power operations for the past three years.

Returns of central station operations received by the *Electrical World* for the month of March, as shown in these tables, clearly indicated a resumption of industrial activity in every section of the country. In spite of the rapidly advancing season and the correspondingly

decreased lighting requirements, the average daily output was 100,000 kilowatt-hours in excess of that reported for January, and 2,200,000 kilowatt-hours in excess of the February output. The estimated output during March of 3,648,000,000 kilowatt-hours was, however, 8 per cent below that of March, 1920; but it must be remembered that the spring of last year saw some of the most intense industrial activity in the history of the nation.

TABLE I

CENTRAL-STATION RETURNS FOR TWELVE MONTHS

	Percentage of installed ratings represented	Kw.-hr. output (companies reporting)			Percentage of installed ratings represented	Revenue from the sale of energy (companies reporting)		
		1920 Thousands	1919 Thousands	Per cent increase		1920 Thousands	1919 Thousands	Per cent increase
April	70	2,603,692	2,098,559	24.1	65	$45,547	$36,541	24.5
May	70	2,631,560	2,179,916	21.2	65	43,888	35,512	23.4
June	71	2,630,831	2,175,645	20.2	66	44,434	35,528	24.5
July	71	2,677,131	2,234,215	20.0	66	46,608	36,265	28.5
Aug.	71	2,769,175	2,303,099	20.1	66	47,410	36,713	29.1
Sept.	71	2,734,179	2,327,460	17.4	66	49,224	38,209	28.5
Oct.	71	2,797,625	2,499,488	11.9	66	51,370	41,055	25.1
Nov.	71	2,741,705	2,503,402	9.6	66	54,620	44,711	22.0
Dec.	71	2,792,554	2,648,746	5.4	66	57,697	48,580	18.6
		1921	1920			1921	1920	
Jan.	76	2,765,632	2,943,349	—6.0	70	58,855	50,861	15.8
Feb.	76	2,456,553	2,711,251	—9.4	70	55,778	48,624	14.8
Mar.	77	2,812,493	3,179,943	—11.6	72	55,706	49,014	13.7

	Percentage of installed ratings represented	Operating and maintenance expenses (companies reporting)			Operating Ratio					
					Steam plants		Hydro plants		Combined systems of steam and hydro	
		1921 Thousands of dollars	1920 Thousands of dollars	Per cent increase	1921	1920	1921	1920	1921	1920
Jan.	40	14,562	13,085	11.3	56.1	55.3	26.8	27.0	38.1	45.6
Feb.	43	15,085	13,640	10.5	55.6	54.5	22.9	26.0	44.1	47.3
Mar.	53	18,544	17,125	8.4	56.1	57.7	23.4	19.6	40.3	44.8

Taking the returns by sections and industrial centers of the country we find, from data issued by federal bureaus, that the general textile situation in New England showed a marked improvement over February. The boot and shoe industry of that section, which accounts for over 50 per cent of the shoe production in the United States, increased operations slightly over February, but was still operating at only about 50 to 75 per cent. From a close study of the companies serving the textile centers of Massachusetts, New Hampshire, and Connecticut, it was found that the operations of these power companies checked these

TABLE II

CENTRAL-STATION RETURNS BY SECTIONS OVER A TWELVE-MONTH PERIOD

Month		New England States			Percentage of installed ratings represented	Atlantic States			Percentage of installed ratings represented	North Central States			Percentage of installed ratings represented	South Central States			Percentage of installed ratings represented	Pacific and Mountain States		
		1920 Thousands	1919 Thousands	Per cent increase		1920 Thousands	1919 Thousands	Per cent increase		1920 Thousands	1919 Thousands	Per cent increase		1920 Thousands	1919 Thousands	Per cent increase		1920 Thousands	1919 Thousands	Per cent increase
Kw.-hr. Output	April	187,174	144,922	29.1	82	883,264	701,778	25.9	74	857,467	687,751	24.7	56	135,140	99,877	35.2	79	540,647	464,231	16.7
	May	188,566	148,186	27.5	82	854,631	711,276	20.0	74	862,127	700,095	23.5	56	137,779	98,815	39.2	79	588,457	521,544	12.6
	June	189,934	149,607	26.5	82	865,714	721,688	19.9	74	855,013	681,780	25.5	56	132,733	99,660	33.2	79	587,437	522,901	12.2
	July	190,668	159,000	20.0	82	881,241	714,485	23.5	74	841,351	702,302	19.7	56	138,937	106,460	30.1	79	614,934	551,959	13.4
	Aug.	194,028	171,002	13.5	82	905,170	728,997	24.2	74	891,712	750,787	18.6	56	147,082	111,573	32.0	79	631,183	541,640	16.5
	Sept.	194,730	170,593	14.1	82	905,872	758,737	19.5	74	878,237	758,017	15.9	56	147,524	113,649	30.0	79	607,816	526,464	15.5
	Oct.	196,310	191,132	2.7	82	964,739	841,694	14.5	74	896,241	817,757	9.5	56	157,267	126,769	24.1	79	583,068	522,136	11.7
	Nov.	193,231	183,024	5.4	82	953,436	825,011	15.5	74	895,229	861,352	10.5	56	144,889	125,438	15.0	79	554,920	507,977	9.2
	Dec.	210,494	227,403	—7.4	82	969,909	917,945	5.7	74	903,628	852,264	5.9	56	140,485	133,419	5.3	79	508,038	517,715	9.6
	Jan.		1921 200,269	1920 247,800	—19.2	81	1921 1,003,223	1920 1,043,650	—3.9	77	73	1921 860,643	1920 956,078	—10.0	62	1921 144,936	1920 145,336	—0.3	81	1921 556,561	1920 550,085	1.1
	Feb.		178,694	216,890	—17.6	77	901,942	960,318	—7.0	73	790,935	883,545	—10.5	63	131,654	132,537	—0.7	81	453,928	508,961	—10.8	
	Mar.		204,952	230,125	—10.9	79	1,028,624	1,117,441	—8.0	73	895,900	943,470	—5.0	63	139,650	136,971	1.9	81	543,367	751,936	—27.2	
Revenue	April		1920 $5,087	1919 $3,943	29.0	82	1920 $17,700	1919 $14,383	23.5	57	1919 $12,690	1919 $10,042	26.9	54	1920 $2,952	1919 $2,181	35.2	79	1920 $7,118	1919 $5,992	19.2	
	May		4,897	3,741	30.1	64	16,624	13,631	21.9	57	12,471	9,853	26.8	54	2,829	2,140	32.0	79	7,067	6,147	15.1	
	June		4,948	3,778	30.5	65	16,525	13,449	22.1	57	12,412	9,776	26.9	54	2,849	2,127	33.9	79	7,700	6,398	20.5	
	July		5,009	3,802	28.5	65	16,899	13,438	25.5	57	12,822	9,793	30.9	54	2,747	2,194	35.0	79	9,131	6,948	31.5	
	Aug.		5,075	4,093	24.0	65	17,210	13,731	25.9	57	13,497	10,151	32.9	54	3,168	2,281	38.1	79	8,460	6,457	31.1	
	Sept.		5,409	4,296	25.9	65	18,081	14,456	24.5	57	13,834	10,522	31.5	54	3,262	2,386	36.5	79	8,638	6,549	32.0	
	Oct.		5,675	4,623	22.7	65	19,201	15,722	22.0	57	14,458	11,667	23.9	54	3,504	2,551	37.3	79	8,532	6,492	31.5	
	Nov.		5,898	5,017	17.5	65	20,938	17,119	22.2	57	15,158	12,561	20.8	54	3,853	2,910	32.4	79	8,775	7,104	23.5	
	Dec.		6,548	5,744	14.0	65	22,332	19,140	16.7	57	16,004	13,142	21.8	54	3,722	3,113	19.8	79	9,091	7,432	22.3	
	Jan.		1921 6,379	1920 6,170	3.5	74	1921 22,180	1920 19,225	15.3	59	1921 16,818	1920 14,436	17.3	62	1921 4,155	1920 3,484	19.3	81	1921 9,323	1920 7,546	23.6	
	Feb.		5,925	5,728	3.4	74	21,489	18,317	17.4	59	15,981	14,071	13.7	62	3,915	3,329	17.6	81	8,468	7,179	18.0	
	Mar.		5,788	5,566	3.8	76	21,761	18,870	15.4	59	15,945	14,108	12.5	63	3,969	3,246	19.2	81	8,489	7,224	17.5	
Operating Expenses:																				
	Jan.	34	1921 $1,008	1920 $1,288	—14.7	48	1921 $6,373	1920 $5,411	17.8	28	1921 $3,941	1920 $3,451	14.2	37	1921 $1,015	1920 $905	12.2	54	1921 $2,135	1920 $2,030	5.2	
	Feb.	35	1,069	1,242	—13.9	53	6,799	5,937	14.5	29	3,958	3,445	14.8	44	1,217	1,056	15.5	55	2,042	1,963	3.9	
	Mar.	46	1,560	1,642	—5.0	59	7,874	7,274	8.2	35	4,597	4,191	9.8	53	1,704	1,453	17.3	79	2,909	2,565	9.4	

government data almost exactly. The companies serving large shoe factories reported an average daily output practically equal to that of February, which fact indicated a slight resumption of activity in the shoe factories, since otherwise the advancing season would have reduced the energy demand by reason of reduced lighting requirements. These and other increases in the industrial load of this section resulted in an increased daily energy output over February of 280,000 kilowatt-hours, and reflected almost perfectly the degree of increased industrial activity.

During March the Atlantic States presented two conflicting tendencies. In the Middle Atlantic States, which group includes the great industrial states of New York, New Jersey, and Pennsylvania, there was a material increase of activity among the industries closely allied with the automobile industries; but on the other hand, the iron and steel industry and other metal products experienced a decidedly greater slump during that month, and the coal-mining districts, such as those of West Virginia, were operating at only about 25 per cent capacity. The reports of light and power companies serving the automobile centers of New York State were materially in excess of those made during previous months in spite of the rapidly advancing season. On the other hand, companies serving the large iron and steel districts, and especially the coal mining districts, showed almost unprecendented drops in electrical output.

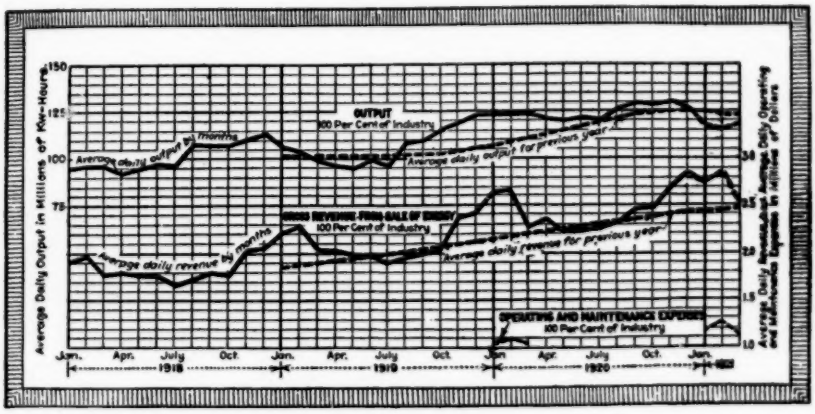
In the South Atlantic States conditions were reported as fast approaching normal in the textile and lumber industries which consume over 46 per cent of the electrical energy generated in the Southern States. Reports from light and power companies operating in the Southern States showed a small increase in the aggregate, which would undoubtedly have been much larger had the decreased lighting load not tended to pull down the gains from an increased industrial power load.

Taking the Atlantic Section as a whole, therefore, we find that the increased power requirements of the South and of a few industries of the Middle Atlantic States were sufficient to overcome the curtailments of the steel and coal-mining industries, resulting in an increased daily output in the Atlantic States of 3,600,000 kilowatt-hours over that reported for February.

Similar conflicting tendencies were also experienced in the North Central States. The greatly decreased power requirements of the iron and steel industry were more than offset by the greatly increased energy requirements of the automobile and related industries, operations in which increased more than 63 per cent over those of February.

But after accounting for the decreased lighting requirements, the resulting energy output of this section was only a trifle more than that reported for February, indicating a slight increase in industrial activity in the aggregate.

Activity in all the copper, lead, and coal mining centers of the Mountain States was considerably curtailed during March, and general industrial activity in this section was at a low ebb. Reports from light and power companies in the Mountain States indicated these very conditions in their subnormal electrical output. Generally speaking, conditions throughout the Pacific States were slightly improved, and the



Central Station operations through March, 1921

increased electrical output of these three states was sufficient to make the aggregate average daily output of the Mountain-Pacific States about 8 per cent greater than that reported in February.

Knowing the location of the various classes of industrial activity, and having at hand a report of the operations of the light and power companies serving these centers, it is entirely possible to plot the degree of industrial activity in any section or industrial center, and for any special industry in that center. Were it possible to obtain detailed reports of the distribution of the central station generating load among the various industries, a study of the monthly operations of the electric light and power industry would give the most accurate story obtainable of industrial activity.

Most government and private reports on industrial conditions are based on returns from a small percentage of the manufacturing plants, and generally it is only the larger plants that make the necessary reports. The electric light and power industry, however, goes into all kinds of

industrial life, both large and small. The interpretations which are to be drawn from the operations of the energy-producing plants tell the story not only of the larger industrial plants but of the small factories also. In a word, the real condition of affairs in the industrial world is accurately pictured by the operations of the electrical industry. And this must be more and more the case each year. The central generating stations cannot install generators and distributing systems fast enough to keep up with the demands of the industrial world for electrical energy.

Each new industrial customer means just one more step toward 100 per cent reflection of activity in all factories. Many plants possibly will never be electrified; but the large proportion which will be electrified will be sufficient to render the monthly reports of electric light and power companies true weathervanes of the storms approaching in the industrial skies and of the lifting of the clouds of depression which may have settled over industry as a whole.

We have a new prophet in our midst. Let us not fail to recognize him and his power and to give to the electric light and power industry the place that it deserves, not only as a great deliverer of mankind from drudgery and darkness, but also as a true prophet of that which is and that which is to come.

THE MATHEMATICAL REPRESENTATION OF FREQUENCY DISTRIBUTIONS

By Harry C. Carver, *University of Michigan*

Section I. distributions of graduated variates*
Section II. distributions of integral variates*
Section III. difference equation graduation
Section IV. application of the hypergeometric series

(A) $u_x = {}_{pn}C_{r-x} \quad {}_{qn}C_x$
(B) $u_x = {}_{pn}H_{r-x} \quad {}_{qn}H_x$

Section III

DIFFERENCE EQUATION GRADUATION

Certain geometrical properties of unimodal frequency distributions suggest that any associated frequency function may be represented as a solution of the difference equation.

$$\frac{\Delta y_x}{\Delta_x} = \frac{y_x(a-x)}{f(x)} \tag{1}$$

since

(a) if there be one mode only there must be a value of $x=a$ for which $\Delta y_x = 0$, and

(b) towards the extremes, the finite difference between two successive ordinates must approach zero as y_x diminishes in value.

The balance of the difference equation of the unknown theoretical law of distribution may be represented by a function, $f(x)$, appearing in the denominator.

We shall now *assume* that $f(x)$ may be expanded in a power series which in practice is found to be rapidly convergent. The merits of this important assumption will be discussed briefly later.

Expressing (1) then as

$$(b_0+b_1x+b_2x^2+ \ . \ . \ . \ )\Delta y_x = (a-x)y_x \cdot \Delta x,$$

multiplying through by x^n and summing with respect to x yields

$$b_0\Sigma x^n\Delta y_x + b_1\Sigma x^{n+1}\Delta y_x + b_2\Sigma x^{n+2}\Delta y_x + \ . \ . \ . \ = (a\Sigma x^n y_x - \Sigma x^{n+1}y_x)\Delta x. \tag{2}$$

* Sections I and II of this paper appeared in the June issue of the Quarterly Publications.

If the range of the distribution be from $x=-\infty$ to $x=\infty$, we have by finite integration by parts

$$\sum_{x=-\infty}^{\infty} x^n \Delta y_x = x^n y_x - \Sigma\left\{(x+\Delta x)^n - x^n\right\} y_{x+\Delta x}\Big]_{x=-\infty}^{\infty}$$

$$= -\Sigma\left\{x^n - (x-\Delta x)^n\right\} y_x\Big]_{x=-\infty}^{\infty}$$

$$(3) \qquad = -nC_1\Sigma x^{n-1} y_x \Delta x + {}_nC_2\Sigma x^{n-2} y_x (\Delta x)^2 - \ \ldots$$

When dealing with distributions of graduated variates, Δx should be permitted to approach zero as a limit.

Thus, equations (1), (2), and (3) become

$$(1a) \qquad \frac{1}{y}\frac{dy}{dx} = \frac{a-x}{b_0+b_1x+b_2x^2+\ \ldots}$$

$$(2a) \qquad b_0\int x^n dy + b_1\int x^{n+1}dy + b_2\int x^{n+2}dy + \ \ldots = a\int x^n y dx - \int x^{n+1} y dx$$

$$(3a) \qquad \int x^n dy = -N \cdot n\nu'_{n-1}.$$

If we choose the mean of the distribution as origin and give n in (2a) successively the values 0, 1, 2, . . . we obtain

$$(4a) \quad \begin{cases} a & +b_1 & + \ \ldots & = 0 \\ & b_0 & +3\nu_2 b_2 + \ \ldots & = \nu_2 \\ \nu_2 a & +3\nu_2 b_1 + 4\nu_3 b_2 + \ \ldots & & = \nu_3 \\ \nu_3 a + 3\nu_2 b_0 + 4\nu_3 b_1 + 5\nu_4 b_2 + \ \ldots & & & = \nu_4 \end{cases}$$

etc.

For distributions of graduated variates we take the common difference between any two successive class magnitudes as the unit for x, (thus $\Delta x = 1$) and giving n successively the values 0, 1, 2, . . . as before we obtain from (2) and (3)

$$(4) \quad \begin{cases} a & & +b_1 & -b_2 + \ \ldots & = 0 \\ & b_0 & -b_1 + & (3\nu_2+1)b_2 + \ \ldots & = \nu_2 \\ \nu_2 a & -b_0 + & (3\nu_2+1)b_1 + & (4\nu_3 - 6\nu_2 - 1)b_2 + \ \ldots & = \nu_3 \\ \nu_3 a + (3\nu_2+1)b_0 + & (4\nu_3 - 6\nu_2 - 1)b_1 + & (5\nu_4 - 10\nu_3 + 10\nu_2 + 1)b_2 + \ \ldots & & = \nu_4 \end{cases}$$

etc.

It should be noted that the moments of (4a) are defined by

$$\nu_n = \int x^n y dx$$

whereas those of (4) are given by

$$\nu_n = \Sigma x^n y_x.$$

A simultaneous solution of equations (4*a*) determines the constants of the *differential* equation (1*a*), which on integration produce Pearson's system of Generalized Probability Curves.

Equations (4) likewise determine the constants for the corresponding *difference* equation (1), when $\Delta x = 1$.

If for a particular distribution the series $b_0 + b_1 x + b_2 x^2 + \ldots$ converges rapidly and it is possible to neglect all terms containing powers of x greater than the second, solutions of (4*a*) and (4) yield, letting

$$\beta_1 = \frac{\nu_3^2}{\nu_2^3} \text{ and } \beta_2 = \frac{\nu_4}{\nu_2^2}.$$

TABLE I

Differential values	Const.	Difference values
$\dfrac{-\frac{\nu_3}{\nu_2}(\beta_2+3)}{2(5\beta_2-6\beta_1-9)}$	a	$\dfrac{-\frac{\nu_3}{\nu_2}\left(\beta_2+3-\frac{1}{\nu_2}\right)}{2\left(5\beta_2-6\beta_1-9+\frac{1}{\nu_2}\right)} - \frac{1}{2}$
$\dfrac{\nu_2(4\beta_2-3\beta_1)}{2(5\beta_2-6\beta_1-9)}$	b_0	$\dfrac{\nu_2\left(4\beta_2-3\beta_1-\frac{1}{\nu_2}\right)}{2\left(5\beta_2-6\beta_1-9+\frac{1}{\nu_2}\right)} - a$
$-a$	b_1	$b_2 - a$
$\dfrac{2\beta_2-3\beta_1-6}{2(5\beta_2-6\beta_1-9)}$	b_2	$\dfrac{2\beta_2-3\beta_1-6+\frac{1}{\nu_2}}{2\left(5\beta_2-6\beta_1-9+\frac{1}{\nu_2}\right)}$

If the series, $f(x)$, converges so rapidly that the term $b_2 x^2$ may also be neglected, that is in cases where the value of b_2 is not appreciably greater than its probable error, we have

TABLE II

Differential values	Const.	Difference values
$-\frac{\nu_3}{2\nu_2}$	a	$\frac{-\nu_3}{2\nu_2} - \frac{1}{2}$
ν_2	b_0	$\nu_2 - a$
$-a$	b_1	$-a$

In application, the difference equation has certain advantages over the differential equation. Thus, a knowledge of the values of the constants of the differential equation permits us to compute the theoretical ordinates only after the integration of the differential equation, the constant of integration being determined by imposing the condition that the sums of the graduated and ungraduated frequencies must be equal.

The difference equation, however, requires no integration, since a knowledge of the constants permits us to compute first all necessary values of $\frac{\Delta y_x}{y_x}$ and then $\frac{y_{x+1}}{y_x}$ from which ordinates proportional to those required may be computed by successive multiplication. The condition that the sum of the graduated frequencies must equal that of the ungraduated determines the proper proportional factor.

For numerical illustrations of the use of the difference equation method for graduating complete distributions as well as "stumps" of distributions, reference may be made to "On the Graduation of Frequency Distributions" by the writer.*

Section IV

Application of the Hypergeometric Series

$$(A) \quad u_x = {}_{pn}C_{r-x} \quad {}_{qn}C_x$$

$$(B) \quad u_x = {}_{pn}H_{r-x} \quad {}_{qn}H_x$$

Critical investigations of the variations which are found to exist in apparently homogeneous statistical data have led to the development of various theories of frequency distribution.

One of the first of these, known to biologists as Quetelet's Law, states that the distribution of individuals ranked according to some common character in a frequency series may be represented by the successive terms of the expansion of the point binomial

$$N(p+q)^r,$$

that is to say

$$(1) \qquad N\left\{p^r + {}_rC_1 p^{r-1}q + {}_rC_2 p^{r-2}q^2 + \ . \ . \ . \ + q^r\right\}$$

where $p+q=1$

N = the total frequency of the distribution.

r = an integer, representing, therefore, one less than the number of classes in the theoretical distribution.

* Published in the *Proceedings of the Casualty Actuarial and Statistical Society of America*, vol. vi, part 1, No. 13.

A student of probabilities, however, is more apt to associate the name of Bernoulli with this series, since the successive terms are merely the frequency expectations of $r, r-1, r-2, \ldots 0$ occurances in N trials of r independent events each, where the probability of the happening of each event is designated by p and of its non-happening by q.

The difference equation of Quetelet's Law, taking the position of the first term as origin, is

$$\frac{y_{x+1}}{y_x} = \frac{N \cdot {}_rC_{x+1} p^{r-x-1} q^{x+1}}{N \cdot {}_rC_x p^{r-x} q^x} = \frac{(r-x)q}{(x+1)p}$$

or

$$\frac{\Delta y_x}{y_x} = \frac{rq-p-x}{(x+1)p}.$$

If the origin be now shifted to the mean, rq units distant, the new difference equation becomes

$$\frac{\Delta y_x}{y_x} = \frac{-p-x}{p(1+rq)+px} \tag{2}$$

which is of the form

$$\frac{\Delta y_x}{y_x} = \frac{a-x}{b_0+b_1 x}.$$

From the values of the constants of the difference equations given in Table II we have

$$p = \frac{\nu_2+\nu_3}{2\nu_2}, \quad q = \frac{\nu_2-\nu_3}{2\nu_2}, \quad r = \frac{4\nu_2^3}{\nu_2^2-\nu_3^2}. \tag{3}$$

From the above we see that if p, q, and r are to have any real significance, the absolute value of ν_3 must be less than ν_2. [Otherwise r would be negative and either p or q would be greater than unity.]

An important limit of Quetelet's Law is obtained by permitting q to approach zero and r infinity in such a manner that the product rq, representing the distance from the origin of the series to the mean, remains a constant and equal to m.

This limit,

$$\frac{N}{e^m}\left[1+m+\frac{m^2}{\underline{|2}}+\ldots+\frac{m^x}{\underline{|x}}+\ldots\right] \tag{4}$$

is known as Poisson's Exponential Binomial Limit, and is often referred to as the Law of Small Numbers.

The criterion for this series is obviously $\nu_2 = \nu_3$.

In the *Philosophical Transactions of the Royal Society of London* (vol. 186, part 1, p. 360), Pearson presents a generalized series which is more general and powerful than the point binomial mentioned above.

It may be developed as follows:

If from a bag containing pn black and qn white balls, r balls are withdrawn without replacements, the chances that the r balls withdrawn will contain $r, r-1, r-2, \ldots 2, 1, 0$ black balls are given by the successive terms of the hypergeometric series

$$(5) \qquad \frac{1}{{}_nC_r}\left\{{}_{pn}C_r + {}_{pn}C_{r-1}\,{}_{qn}C_1 + {}_{pn}C_{r-2}\,{}_{qn}C_2 + \ldots + {}_{qn}C_r\right\}.$$

The difference equation of this series, referred to the mean which is rq units distant from the first term, is

$$(6) \qquad \frac{\Delta y_x}{y_x} = \frac{(r\sqrt{2p-1} - pn - 1) - (n+2)x}{(r\sqrt{1-p} + 1 + x)(p\sqrt{n-r} + 1 + x)}.$$

Comparing equation (6) with (4) of Section III, we obtain the following for the hypergeometric series $u_x = {}_{pn}C_{r-x}\,{}_{qn}C_x$:

$$(7) \quad \begin{cases} \nu_2 = rpq\dfrac{n-r}{n-1} \\ \nu_3 = \nu_2(p-q)\dfrac{n-2r}{n-2} \\ \nu_4 = \dfrac{\nu_2}{(n-2)(n-3)}\left\{3(n-1)(n+6-\dfrac{2}{pq})\nu_2 + n(n+1) - 6pqn^2\right\}. \end{cases}$$

But here again we find that for distributions of integral variates our results are unintelligible unless ν_3 is in absolute value less than ν_2.

Again, since for this series $b_2 = \dfrac{1}{n+2}$ we have from Table I,

$$(8) \qquad n = \frac{6(\beta_2 - \beta_1 - 1)}{2\beta_2 - 3\beta_1 - 6 + \dfrac{1}{\nu_2}};$$

and a few trials will convince one that for many of the distributions that are met in practice this solution yields a negative value for n, and that this occurs when $\nu_3 > \nu_2$.

If we now consider the hypergeometric series

$$u_x = {}_{pn}H_{r-x} \cdot {}_{qn}H_x$$

where ${}_nH_r$ denotes the number of combinations of n things taken r at a time when repetitions are allowed, *i. e.*, ${}_nH_r = {}_{n+r-1}C_r$, we have

$$\frac{\Delta y_x}{y_x} = \frac{(r\overline{1-2p}+1-pn)-(n-2)x}{(r\overline{1-p}+1+x)(p\overline{n+r}-1-x)}, \tag{9}$$

where the mean, which is also at rq, is taken as the origin.

Comparing equation (9) with (4) of Section III we obtain for the series $u_x = {}_{pn}H_{r-x} \cdot {}_{qn}H_x$

$$\begin{cases} \nu_2 = rpq\dfrac{n+r}{n+1} \\ \nu_3 = \nu_2(p-q)\dfrac{n+2r}{n+2} \\ \nu_4 = \dfrac{\nu_2}{(n+2)(n+3)}\left\{3(n+1)(n-6+\dfrac{2}{pq})\nu_2+n(n-1)-6pqn^2\right\}. \end{cases} \tag{10}$$

It may now be noted that equations (9) and (10) may be obtained directly from (6) and (7) by replacing n in the latter by $(-n)$.

If for convenience we designate the point binomial or Bernoulli's series by Series B, the hypergeometric series $u_x = {}_{pn}C_{r-x\,qn}C_x$ as Series C, and $u_x = {}_{pn}H_{r-x\,qn}H_x$ as Series H, we see that Series H may be used for those distributions for which Series B and C are meaningless.

An analysis of the means and dispersions of these three series is enlightening. From the following table we note that although their

TABLE III

Series	Mean	Dispersion
C	rq	$rpq\frac{n-r}{n-1}$
B	rq	rpq
H	rq	$rpq\frac{n+r}{n+1}$

means are identical the dispersion of Series C is always less and that of Series H greater than the Bernoullian dispersion. Moreover, as n approaches infinity, the dispersions of Series C and H approach the Bernoullian as a limit from opposite sides.

Inasmuch as the Bernoulli Series, because of its rather extensive degree of freedom, is itself a powerful "closed" graduation function, it follows that the combination of these three series—affording an addi-

tional continuous degree of freedom—is capable of graduating practically any unimodal distribution.

These considerations throw an interesting light on the convergence of $f(x)=b_0+b_1x+b_2x^2+\ \ldots$ in the denominator of either the difference or differential equation. If we stop with b_1x the freedom is restricted to that of a point binomial, and the addition of b_2x^2 increases the freedom to at least that of the hypergeometric series.

At the present time tables, based on formulae (7) and (10), are being prepared which will enable one to obtain by inspection the proper values of p, q, r, and n when the values of the moments are known. By this method it is hoped that a simple method of graduating frequency distributions may be available, and, what is more important, that something may be accomplished in the direction of classifying distributions.

NOTES

AN EXAMINATION OF THE WAGE STUDIES OF THE NATIONAL INDUSTRIAL CONFERENCE BOARD

By Paul H. Douglas, *University of Chicago*

Wartime Changes in Wages. September 1914–March 1919. National Industrial Conference Board, Research Report No. 20, 128 pp.

Changes in Wages During and Since the War. September 1914–March 1920. National Industrial Conference Board, Research Report No. 31, 53 pp.

I

In these two studies the National Industrial Conference Board has attempted to survey the movement of money wages since September, 1914, in a number of industries. The first report covered the following manufacturing industries: metal, cotton, woolen, silk, boots and shoes, paper, rubber, and chemicals; the second added furniture, hosiery and knit goods, printing and publishing, and leather tanning.

The earlier study concerned itself with only two methods of computing wages: (1) actual weekly earnings, *i. e.*, the amounts actually received in the pay envelopes; (2) the hourly earnings, *i. e.*, the actual weekly earnings divided by the number of hours actually worked. Hourly earnings should be clearly distinguished from hourly wage rates in that they include their portion of any bonuses that may have been paid the worker. To illustrate: An employee with an hourly rate of fifty cents works sixty hours a week and receives not only sixty hours' pay but a bonus of ten hours' pay in addition for overtime. His weekly earnings are therefore $35.00 and his average hourly earnings one-sixtieth of this, or 58.3 cents, whereas his wage rate is only 50 cents an hour. The Board states that the investigations of the National Metal Trades Association showed that "averages of hourly earnings . . . generally exceeded the corresponding averages of hourly rates by from two to five cents."* The later investigation introduced a further method, namely, that of full-time weekly earnings, *i. e.*, the hourly earnings multiplied by the number of hours considered as the standard week's work. Thus, a plant operating under the forty-eight

* Research Report No. 31, p. 1.

hour week would multiply the average hourly earnings by that figure. It is clear that this third basis is not one of actual earnings, since it makes no allowance for unemployment, underemployment, or overtime. It would naturally exceed the actual weekly earnings in those cases in which the number of hours worked was less than the standard, and would fall below them when overtime was worked. It was indeed this third basis upon which the Board leaned the most heavily in the second report, although data on this point were missing for the paper and chemical industries.

The second report broadened the scope of investigation in still another way; it included additional plants and consequently more employees. Thus, the second investigation covers for September, 1918, approximately 13,000 more employees in the metal industry than did the first, 4,000 more in the cotton industry, 20,000 more in woolens, and certain increases in other occupations as well.

One difficulty with the studies is their lack of a general summary. The first eschewed even finding a percentage increase for any industry as a whole, and contented itself with merely showing the increase by sexes and by occupations within each industry. The second emphasizes the comparison by sexes, but shows the increase for each of the crafts studied for the single date of March, 1920, as compared with September, 1914, and then only for hourly earnings and full-time weekly earnings—not for actual weekly earnings. Following the partial example of the report, the reviewer has employed a similar method and has weighted the indices given for the sexes in each industry by the number of each sex.* By this method he has secured the following indices for all three bases of wage computation both for March, 1920, and for September, 1918. From these indices, index numbers for the combined twelve industries have been computed. One is the simple arithmetic average of the indices for the separate industries, while the other is obtained by weighting the indices for each industry by the number of employees included in the wage study for that industry. It is not at all certain that the second method is under the circumstances more accurate than the first, since the relative numbers examined by the Board in the various industries does not necessarily correspond to the relative actual number employed in the country as a whole. Thus, the study made by the Board of the rubber industry in March, 1920, applied to 63,000 workers, while that for printing and publishing covered only 1,400. The weighted index, therefore, gives rubber forty-five times the importance attached to printing and publishing. Both

* The data for the chemical and furniture industries applied only to men. Hence weighting was not necessary here.

the weighted and unweighted index numbers, however, are given for use.

TABLE I

RELATIVE WAGES—ALL WORKERS

(September 1914 = 100)

Industry	Hourly earnings		Actual weekly earnings		Full-time weekly earnings	
	September, 1918	March, 1920	September, 1918	March, 1920	September, 1918	March, 1920
Metal manufacturing	179	210	196	213	170	191
Cotton manufacturing	205	272	206	251	206	236
Woolen manufacturing	204	272	203	252	203	240
Silk manufacturing	188	282	191	259	189	263
Boots and shoes	160	205	157	187	145	182
Paper manufacturing	179	213	179	220	...	...
Rubber manufacturing	197	267	179	238	197	243
Chemical manufacturing	175	261	193	273	...	...
Furniture manufacturing	158	217	161	212	158	194
Hosiery and knit goods	191	274	229	259	203	250
Leather tanning	195	270	212	274	191	236
Printing and publishing	138	196	130	186	129	185
Unweighted average	181	245	186	235	179	222
Weighted average	186	250	193	234	182	226

In order to translate these indicated increases of money wages into relative real wages, it is of course necessary to compare them with the increase in the cost of living during these periods. The three sources from which we can derive cost of living figures for approximately this period are: (1) the studies of the National Industrial Conference Board itself; (2) an average of the cost of living indices obtained by the United States Bureau of Labor Statistics for eighteen cities; and (3) the cost of living index for Massachusetts computed by the Special Commission on the Necessaries of Life of that state. This comparison is shown in the following table:

INCREASE IN THE COST OF LIVING* 1914–MARCH, 1920

	Bureau of Labor Statistics Eighteen cities December, 1914 = 100	National Industrial Conference Board July, 1914 = 100	Massachusetts Commission September, 1914 = 100
September, 1918	164.6	158.6	158.0
March, 1920	205.0	194.8	187.4

* The average increase in the cost of living for the eighteen cities is given for December, 1917, December, 1918, December, 1919, and June, 1920. The figures for September, 1918, and March, 1920, were secured on the assumption that the increase was evenly distributed through the months between the dates for which the figures were at hand. The same method was used with the statistics of the National Industrial Conference Board to secure the figure for September, 1918.

It will be seen that the figures of the Massachusetts Commission and the National Industrial Conference Board are from six to eighteen points below those of the Bureau of Labor Statistics. In all probability, however, the data of the Bureau of Labor Statistics are more reliable for the larger cities than are those of the other two. The situation in Massachusetts was undoubtedly not typical of the country as a whole, for that state had an oversupply of houses at the outbreak of the war and rents in consequence rose much more slowly there than in other parts of the country. And it is doubtful whether the investigations of the National Industrial Conference Board were made as carefully or as accurately as those of the Bureau.

We may now compare the various indices of money wages as computed by the National Industrial Conference Board with the indices of the cost of living, and secure the consequent indices of real wages.

TABLE II

PURCHASING POWER OF WAGES BASED ON WAGE STATISTICS OF NATIONAL INDUSTRIAL CONFERENCE BOARD

(September, 1914 = 100)

Basis of wage computation	September, 1918			March, 1920		
	Bureau of Labor Statistics index	Massachusetts Commission index	National Industrial Conference Board index	Bureau of Labor Statistics index	Massachusetts Commission index	National Industrial Conference Board index
HOURLY EARNINGS						
Simple average	110	114	114	120	126	131
Weighted average	113	117	117	122	128	133
ACTUAL WEEKLY EARNINGS						
Simple average	113	117	117	115	121	125
Weighted average	118	122	122	114	120	125
FULL-TIME WEEKLY EARNINGS						
Simple average	109	113	113	108	114	118
Weighted average	111	115	115	110	116	120

This apparently shows an increase in real wages of approximately 10 per cent during the war, by whatever method of computation is used. This result is so greatly at variance with other studies of wages in war time,* which showed on the whole a decline in real wages, that it should be given the closest scrutiny before being accepted.

II

Fortunately, the investigations of the National Industrial Conference Board can be checked up with those of the United States Bureau of

* See the article by Frances Lamberson and myself, "The Movement of Real Wages, 1890–1918," in the *American Economic Review*, September, 1921.

Labor Statistics from 1914 to 1918 with regard to hourly earnings and full-time weekly earnings in three of the industries; namely, boots and shoes, woolen and worsted manufacturing, and cotton manufacturing. Bulletins 260, 261, and 262 respectively of the Bureau of Labor Statistics contain the data for the industries. These data were derived from actual payrolls, as was also that of the National Industrial Conference Board, and the definition given by each to "hourly earnings" and "full-time weekly earnings" is the same. The bulletins of the Bureau of Labor Statistics do not give a wage index for each industry as a whole, but they do give indices for practically all of the various occupations within the industry. The reviewer has, therefore, (1) taken the wage index for each *occupation* wherever given, and has shifted the base upon which the 1918 wage is computed to 1914; (2) computed an index for 1918 on a 1914 base from the actual money wages shown in the bulletins for those occupations for which an index was not given; (3) weighted the index for each occupation by the number employed.* By this method the relative increase in wages has been secured for each industry from 1914 to 1918 for hourly earnings and for full-time weekly earnings, the results of which are shown in Table III in comparison with those of the National Industrial Conference Board.

TABLE III

COMPARISON OF WAGE INCREASES IN 1918 OVER 1914 AS SHOWN BY THE INVESTIGATIONS OF THE NATIONAL INDUSTRIAL CONFERENCE BOARD AND THE UNITED STATES BUREAU OF LABOR STATISTICS

Industry	Hourly earnings			Full-time weekly earnings		
	Increase as per National Industrial Conference Board	Increase as per United States Bureau of Labor Statistics	Relation of National Industrial Conference Board to Bureau of Labor Statistics	Increase as per National Industrial Conference Board	Increase as per United States Bureau of Labor Statistics	Relation of National Industrial Conference Board to Bureau of Labor Statistics
Boots and shoes	60	39.6	+20.4	45	33.3	+11.7
Woolen and worsted manufacturing	104	86.7	+17.3	103	85.1	+17.9
Cotton manufacturing	105	75.5	+29.5	106	72.6	+33.4

A comparison of the statistics for these three industries, therefore, shows that the National Industrial Conference Board's figures for increases in hourly earnings are from 17.3 per cent to 29.5 per cent in

* The National Industrial Conference Board also weighted each occupation by the number employed in order to get a figure for each industry as a whole.

excess of those of the Bureau of Labor Statistics, and for full-time weekly earnings, from 11.7 per cent to 33.4 per cent higher.

Before attempting to determine the possible reasons for this difference, let us analyze the nature of the disparity somewhat more closely. In doing this the writer has compared the money wages for 1914 and 1918 for the various occupations within each industry as given by the National Industrial Conference Board, with the money wages listed for that occupation by the Bureau of Labor Statistics. Considerable difficulty has been experienced in doing this because of the different nomenclature used by the two organizations; but this has been largely overcome, and a comparison of many occupations has been accomplished. The detailed analysis that was made is too cumbersome for inclusion in this review, but the results shown may be briefly summarized:

(1) In the boot and shoe industry, the hourly earnings in 1914 as listed by the National Industrial Conference Board were, with only three exceptions,* from two to six cents below those given by the Bureau of Labor Statistics, and averaged a full 12 per cent less. The full-time weekly earnings in 1914 as shown by the Board were (with one exception) from $1.35 to $4.70 less than those given by the Bureau, and ranged from 10 to 25 per cent less. By 1918, however, the hourly earnings, according to the Conference Board, were (with two exceptions) from one to six cents *above* those of the Bureau, and averaged approximately 6 per cent more. Full-time weekly earnings as shown by the Conference Board also in general exceeded those of the Bureau.

(2) In the cotton manufacturing industry, the 1914 hourly earnings shown by the Conference Board (with two exceptions) exceeded those shown by the Bureau by from one to three cents, and by approximately 8 per cent. By 1918, however, the excess for the occupations compared ranged from three to twelve cents, with an average of about 28 per cent. The Board's 1914 figures for full-time weekly earnings were on the whole no higher than those of the Bureau; but by 1918 they ranged (with two exceptions) from two to five dollars more, or something over 20 per cent higher.

(3) In the woolen industry, the hourly earnings in 1914 as given by the Board were in general somewhat lower than those of the Bureau; but by 1918 they were, save for sorting, spinning, and female weavers, higher. Sorting and spinning earnings, however, were still much lower than in the corresponding scale of the Bureau. The full-time weekly earnings shown by the Conference Board were slightly lower than those

* One of these was more than the Bureau of Labor Statistics' figure, while two were less but not as much less as two cents an hour.

of the Bureau in 1914, but by 1918 they were (with two exceptions) from $2 to $13 more, with a general average of approximately 12 per cent higher.

How, then, may these discrepancies between the investigations of the National Industrial Conference Board and those of the Bureau of Labor Statistics be accounted for, and which is the more reliable?

(1) The periods covered by the two studies are slightly different; the comparisons of the Conference Board are made between the months of September, 1914, and September, 1918, while those of the Bureau are between the average for the whole year 1914 and the average for the year 1918. It is probable that owing to the depression caused by the outbreak of the Great War in August, 1914, earnings in September of that year were slightly lower than their average for the year as a whole. Furthermore, it is probably also true that in the year 1918, characterized as it was by fast-mounting money wages, the earnings for September were somewhat higher than the average of the year taken as a whole. This might partially explain the greater increase shown by the Conference Board.

(2) The samples taken by the Conference Board were considerably smaller than those taken by the Bureau. Thus, in the cotton manufacturing industry the Board drew its statistics from the payrolls of 13,900 employees, while the Bureau's study covered 91,000 employees. In the boot and shoe industry the Board based its study upon 13,500 employees, and the Bureau upon 61,000. In woolen manufacturing the Board's figures for 1918 were based upon 37,000 employees, while those of the Bureau covered 51,000.

(3) The samples of the Conference Board were less widely distributed geographically than those of the Bureau. The Board's statistics for cotton manufacturing were drawn only from the six states of Maine, Massachusetts, Connecticut, Rhode Island, New York, and New Jersey. The Bureau included not only these states but New Hampshire and (most important of all) the Southern States as well. In the boot and shoe industry the Conference Board's figures are based on returns from eight states, while those of the Bureau are based on at least twelve.* In woolen manufacturing, however, the states included in the two investigations are identical.

(4) The plants investigated by the Conference Board were undoubtedly almost overwhelmingly non-union, while those of the Bureau included union as well as non-union plants. The membership of the Conference Board is predominantly composed of employers

* Three factories are listed merely as being located in "other states." There is a possibility therefore that fourteen states are included.

operating non-union shops, and it is from their members that practically all of their wage statistics are taken. Now it is undoubtedly true in general that wages in non-union plants in 1914 were lower than those in union plants. Moreover, it is also probable that during the war wages in the non-union plants rose more rapidly than those in the union shops because of the great competition for labor and the fact that there were no previously agreed upon wage-rates, as in union shops, to retard the upward movement of wages. It is probable, therefore, that these factors in turn will account in part for the discrepancy between the two sets of studies.

(5) The methods used to collect data by the two bodies were different. The National Industrial Conference Board sent out requests for payrolls, and the plants that complied mailed them to the Board. The Bureau of Labor Statistics, on the other hand, sent out agents who personally secured the payrolls from the establishments, and made their computations from this material. There can be no question that the latter method is scientifically superior to the former. In the first place, there is a much greater likelihood of securing more representative returns through personal contact than through a long-distance request. Concerns with a low wage scale are especially reluctant to report their rates of payment. Secondly, there is a much greater opportunity to straighten out any difficulties that may present themselves in the material, and thus to clear up any obscurities. Therefore, although the method of collecting material followed by the Conference Board in the matter of wages is not open to the criticisms that can justly be leveled at their hours and output series, it is distinctly inferior to the method employed by the Bureau of Labor.

From the foregoing factors can we not conclude, then, that the apparent rise in real wages up to 1918 as shown by the National Industrial Conference Board has not been established? The writer believes, upon the basis of a rather extended investigation which he has directed, that from 1914 to 1918 the purchasing power of hourly wage rates decreased appreciably. He is still in doubt as to whether the purchasing power of actual weekly earnings including overtime increased or decreased during that time. During 1919 and the early part of 1920, however, he is inclined to agree that wages and wage rates perhaps rose faster than the cost of living.*

III

Finally, turning from the question of relative wages, if we accept the wage statistics of the Conference Board at their face value, we can

* See the article by Miss Lamberson and myself that has previously been referred to.

secure interesting sidelights upon the question of the adequacy of the wages paid in terms of the various accepted standards of living.

TABLE IV

AVERAGE EARNINGS OF MALE EMPLOYEES IN TWELVE INDUSTRIES AS REPORTED BY THE NATIONAL INDUSTRIAL CONFERENCE BOARD

Industry	September, 1918		March, 1920	
	Average actual weekly earnings (a)	Average yearly earnings (a ×52)	Average actual weekly earnings (c)	Average yearly earnings (c×52)
Boots and shoes	$23.62	$1,228	$28.70	$1,492
Chemical manufacturing	25.24	1,312	35.72	1,857
Cotton manufacturing	20.50	1,066	24.87	1,293
Furniture manufacturing	17.39	904	22.87	1,189
Hosiery and knit goods	22.50	1,170	27.65	1,438
Leather	23.36	1,215	30.18	1,569
Metal manufacturing	27.73	1,442	29.79	1,549
Paper manufacturing	23.20	1,206	28.82	1,499
Printing and publishing	23.69	1,232	31.67	1,647
Rubber manufacturing	27.93	1,452	36.32	1,889
Silk manufacturing	21.48	1,117	28.98	1,507
Wool manufacturing	22.93	1,192	28.70	1,492

Table IV shows the average actual weekly earnings in September, 1918, and March, 1920, for males, and what the average yearly wage would have been at this rate had fifty-two weeks been worked.

When these wage statistics are compared with the amounts necessary to maintain a family of five at these times, still further proof is furnished of the inadequacy of the average adult male's wage to support family life. The most conservative and the most carefully worked out budget for the fall of 1918 is that of the Philadelphia Bureau of Municipal Research* which fixed $1,637 as the amount necessary to support a family of five on "a minimum standard of health and comfort." It should be noted that this standard is in reality much lower than the "standard of health and decency" which has been followed by the United States Bureau of Labor Statistics.† The Philadelphia budget is indeed probably only slightly above a minimum of subsistence. This Philadelphia quantity budget was revised in November, 1919, and in August, 1920, being increased to $1,803 in the former month and $1,988 in the latter.‡ If we assume that the increase was evenly distributed throughout these nine months, we would have a figure of $1,885 as the necessary amount in March, 1920. If we compare the average yearly wages for the various industries as given in Table IV (on the basis of fifty-two weeks' work) with this budget, we see very

* W. C. Beyer and others, *Workingman's Standard of Living in Philadelphia.*

† See the budgets collected by the Bureau of Applied Economics: *Standards of Living.* 1920 edition.

‡ *Citizens Business*, published by Philadelphia Bureau of Municipal Research, No. 463, p. 4.

clearly the inadequacy of the average wages of males, even during the periods of greatest business activity, adequately to support a family.

TABLE V

A COMPARISON OF AVERAGE FULL-TIME YEARLY EARNINGS WITH THE AMOUNT NECESSARY TO MAINTAIN THE PHILADELPHIA STANDARD

Industry	September, 1918		March, 1920	
	Amount needed to bring full-time yearly earnings to minimum	Per cent increase needed to bring full-time yearly earnings to minimum	Amount needed to bring full-time yearly earnings to minimum	Per cent increase needed to bring full-time yearly earnings to minimum
Boots and shoes	$409	33	$393	26
Chemical manufacturing	325	26	28	2
Cotton manufacturing	571	54	592	46
Furniture manufacturing	733	81	696	59
Hosiery and knit goods	467	40	447	31
Leather	422	35	316	20
Metal manufacturing	195	14	336	22
Paper manufacturing	431	36	386	26
Printing and publishing	405	33	238	14
Rubber manufacturing	185	13	4*	..
Silk manufacturing	520	47	378	25
Wool manufacturing	445	37	393	26

* Indicates excess.

Table V shows that in the fall of 1918, the average wage in every one of these industries was much less than the amount required to support a family. This deficiency, save for metal and rubber manufacturing, ranged from three hundred to over seven hundred dollars. An increase of from 23 to 81 per cent was needed to bring the average earnings in ten of the twelve industries up to the minimum. By 1920 the situation was somewhat improved, but even then in only one industry would the full-time yearly earnings have been sufficient for family support. In nine of the twelve industries the gap between the yearly rate of earnings and the minimum was over three hundred dollars, and in one it was nearly seven hundred dollars. In these nine industries increases ranging from 22 to 59 per cent were needed to bring the average yearly rate up to the minimum.

Now three objections may be urged against drawing the conclusion from the above that wages are inadequate: (1) That the standard of living chosen applied only to Philadelphia and cannot be used as a standard for the rest of the country. It is of course true that the cost of living does vary from locality to locality, but it is also undoubtedly true that these differences, at least in industrial centers, are not so great as are commonly supposed. Moreover, we have no information that would lead us to infer that the cost of living in Philadelphia is less

than in other manufacturing centers. (2) That the earnings of the male should not necessarily cover the cost of living for the family as a whole, since it is only proper to expect the family to receive income from other sources. This is a proposition to which one may give a qualified assent subject to judgment as to the specific source of income. Thus, while the work of the wife outside the home is sometimes proper when there are no children, it should be indulged in only in the rarest circumstances when there are children. The contributions of children over sixteen or indeed over fourteen may also be proper. Income from boarders is a source upon which a general conclusion is impossible since although it may be proper in some instances, in many others it is not. In general, however, we should beware of making total family income the test of the adequacy of the wage. The attempts of the family to secure supplementary income may indeed seriously impair the family life and vitality. (3) That since many men have either no dependents or less than four we should not use the family of five as the standard by which to measure the adequacy of men's wages, but should instead use a smaller family. A great deal of research is needed to determine how well-grounded such a contention is. At the present time we lack adequate information as to the proportion of adult males in the United States who have such a standard family, as well as the proportion that have more and those that have fewer dependents. Pending such fuller research and information, it does not seem wise to discard for the present at least the currently accepted standard in favor of a smaller family, particularly in view of the fact that even the present standard does not include (a) the initial cost of family furniture and fittings; (b) the expenses connected with the birth of children; (c) an adequate protection against the risks to which the worker is subjected; and (d) a proper provision for a human depreciation fund to protect the worker against industrial old age.

Although the above points might lead one to suppose that the indicated inadequacy of the wage is an overstatement, the following factors operate in the opposite direction: (1) The standard of living chosen was a most conservative one and one which many experts would increase. (2) The estimated yearly earnings were based upon working the full fifty-two weeks during the year at the same rate as during the period studied. This makes no allowance for unemployment save for such broken time as occurred during the period under consideration. When we remember that the amount of unemployment over a period of years is probably at least 10 per cent, this consideration assumes great importance. (3) The periods chosen were characterized by a great deal of overtime which would be non-existent at other points in the business

cycle when many even of those regularly employed would work only part-time. (4) Finally, it should be remembered that the above figures were only averages. There was, therefore, a very large body of workers who received less than the amounts indicated.

Although it is manifestly impossible to balance these unknown quantities with any degree of even approximate accuracy, it does seem most probable from the figures of the National Industrial Conference Board itself that during the most outwardly prosperous period in our recent economic history a very large section of our working population did not receive a wage high enough to maintain a "decent" standard of living.

A CRITIQUE OF COST-OF-LIVING STUDIES

By George E. Barnett, *Johns Hopkins University*

The rapid changes in retail prices in recent years have since 1917 made the cost of living the dominant consideration in wage adjustments. The importance of statistical measures of these changes has correspondingly increased. In this country the first continuous index numbers of the total cost of living were inaugurated by the Bureau of Labor Statistics in 1917. They showed changes in the total cost of living separately for each of a group of fourteen cities in which shipbuilding was being carried on. At a later date similar index numbers were made for a number of other large cities. The material used in making the separate city indices was still later combined into an index number for the country as a whole.

Reports of its studies of the cost of living were first published by the Industrial Conference Board in August, 1918.* The Board does not calculate index numbers for separate cities, but only an index number of the cost of living in the United States as a whole. Since the index numbers for the United States issued by the Bureau of Labor Statistics and the Industrial Conference Board give figures for exactly the same thing, and since the question as to which gives the more nearly correct result is important, it seems desirable, in place of a mere technical description of the Board's index number, to compare the two index numbers as to result and method.

The difficulty in making such a comparison is that the dates for which the two index numbers are calculated are not identical. The

* *War Time Changes in the Cost of Living*, Research Reports Nos. 9, 14, 17, National Industrial Conference Board.

Changes in the Cost of Living, Research Reports Nos. 19, 25, 28, 30, 33, 36, National Industrial Conference Board.

Comparisons are made with certain studies of the United States Bureau of Labor Statistics.

index numbers of the Bureau have been issued for the following dates: December, 1914, 1915, 1916, 1917, 1918, and June and December, 1919, 1920. The Board's index numbers are for July, 1915, 1916, 1917; June and November, 1918; March, July, and November, 1919; March, July, and November, 1920; and March, 1921. In addition, a monthly estimate of change in the cost of living is made by the Board, but this is subject to revision. Any comparison of the two series must assume, therefore, that changes are continuous between the dates for which the numbers are calculated. The simplest assumption is that the change between any two dates has been by a uniform amount each month. This assumption may not be valid for any single period, but for the whole time there is a strong probability that it is substantially correct.

When the two index numbers are studied in this manner, it appears that the Board's index number shows smaller changes in the cost of living over the initial period at every date than the Bureau's index number. Until August, 1918, the difference was not large, at no time exceeding five points; but since that date it has been between ten and fifteen points. In November, 1920, for example, the Board's index number stood at 193.1, while the index number of the Bureau for December, 1920, was 200.4. According to the monthly estimate issued by the Board, the index number for December was 190.

The first explanation of this disagreement which suggests itself is the difference in the bases from which the changes are calculated. The Board's index number shows changes from July, 1914, but the base of the index of food prices—a very important part of the total—is the average of food prices for 1913. The Bureau's number has the year 1913 as its base. But this difference is not very important. The Bureau's index number shows an increase of only 3 per cent in the cost of living from 1913 to December, 1914, and over half of this increase was due to changes in food prices. The cost of living in July, 1914, was probably, therefore, not more than 1 or at most 2 per cent above the average for 1913. Obviously, the large disagreement between the two index numbers since 1918 cannot be explained by the difference in bases, although this difference lessens somewhat the amount of disagreement to be explained.

Another explanation of the disagreement has been advanced by Dr. Hugh S. Hanna in the October, 1919, number of the *Monthly Labor Review*. Dr. Hanna's article was written before the Bureau began to publish its index number for the United States as a whole, but he uses the same data in compiling his index number. According to his theory the disagreement is due to the fact that the earlier cost-of-living studies

made by the Bureau were entirely in ship-building cities, while the Board's studies covered a much larger number of cities. He estimated from studies made by the Bureau in 1918 that prices had increased from January 1, 1917, 12 per cent less rapidly in other cities than in the ship-building cities. When allowance was made for this fact, the two series agreed fairly well. This explanation cannot, however, be applied with much force to the index numbers of the Bureau after December, 1917, for since that date the Bureau has used data for a number of other cities in addition to the material for the shipbuilding cities, and it is exactly in this period that the discrepancy between the two index numbers is greatest.

Another possible source of the disagreement is suggested by the differences in the weights given to the groups of expenditure. The Bureau's index number is weighted on the basis of the results of a survey made in twenty-two cities between July 31 and November 30, 1918. The weights used by the Board are averages of the results of a number of private and official budgetary studies made at various dates from 1901 to 1917, weighted according to the number of families covered by the respective studies. Since the most extensive of these investigations was the 1901 study of the Bureau, the weights used by the Board are nearly the same as the figures given by that study. In brief, then, the Board's index number is weighted by a distribution of expenditure found in 1901 and the Bureau's number is weighted by a distribution of expenditure found in 1918.* The two systems of weights are as follows:

PER CENT OF THE TOTAL EXPENDITURE

	Board	Bureau
Food	43.13	38.2
Clothing	13.21	16.6
Shelter	17.65	13.4
Fuel and light	5.63	5.3
Sundries	20.38	26.4†

It will be noted that in the Board's system of weighting relatively greater importance is attached to food and shelter, while less importance is attributed to clothing and sundries. If the prices in all groups had advanced equally, the system of weighting adopted, obviously, would have been a matter of indifference. On the other hand, if food and shelter had advanced at a more rapid pace than clothing and sundries, or vice versa, a considerable difference in the index numbers would have

* Neither, of course, the best system of weighting; the ideal system would be one based on the distribution of expenditure in 1913 or 1914.

† In the Bureau's classification of expenditures, sundries are divided into "miscellaneous," with a weight of 21.3, and "furniture and furnishings," with a weight of 5.1.

been attributable to the differences in weighting. As a matter of fact, however, the changes in the prices of the different groups have been of such a compensatory character as largely to nullify the importance of the difference in weighting. Even at the dates where the disagreement due to this cause is greatest, it does not appear to be more than a few points, and it is variable in amount.

It is obvious that none of the differences in construction which have been noted is sufficient to account for the regular, and, since January 1, 1918, fairly large disagreement between the two index numbers. An examination of the index numbers for the component groups of expenditure proves to be a more satisfactory clue. The index numbers for food and fuel and light in the Board's calculation agree with the index numbers for the same groups in the Bureau's calculation. The Board uses the index number for food as calculated monthly by the Bureau. The Board's index number for fuel and light is based on questionnaires sent to a large number of cities. "The changes in the cost of anthracite and bituminous coal are weighted according to their importance for domestic use," and the combined figure for fuel and light is obtained by using a system of weights based on the assumption that "about one-third of the average family expenditure for fuel and light goes for light and about two-thirds for fuel." The Bureau of Labor Statistics has never published any information, as far as the writer is aware, regarding the methods by which it computes its index number for fuel and light. However, since the two index numbers agree fairly well, it is useless for our present purpose to conjecture as to differences in methods.

When we examine the three other groups of expenditure—clothing, housing and sundries—we find not only that the two sets of index numbers do not agree, but that the differences between them are of a fairly regular amount. The index number of the Board for shelter is higher than that of the Bureau for housing, while the index numbers of the Board for clothing and for sundries* are considerably below those of the Bureau. The sum of the differences in the index numbers for clothing and sundries *minus* the difference in the index number for housing, when weighted by the value of these groups in the total cost of living, gives an amount which accounts approximately for the disagreement between the index numbers of the total cost of living.

The differences between the discordant group index numbers are so constant as to suggest fundamental dissimilarities either in material or method. Unfortunately, the information disclosed by the makers of

* The Bureau's index numbers for miscellaneous and "furniture and furnishings" have been combined into a single index number, which is here regarded as comparable with the sundries group of the Board.

these index numbers is not sufficient to make it possible to probe the question thoroughly. In what follows, therefore, only the most superficial analysis has been made.

The index number for shelter used by the Board, as has been said, runs uniformly higher than that of the Bureau for the same expenditure. It is possible to carry the analysis of this index number a step back, since the changes in the cost of housing are given for individual cities by both the Bureau and the Board. When these changes are compared, it becomes clear that the reason for the difference in the index numbers lies primarily, if not entirely, not in the method of averaging the increases in different cities, but in the differences in the amounts of change found by the Board and the Bureau for the same cities. The Board's figures are almost without exception higher than those of the Bureau. For example, the increase in the cost of housing in Philadelphia is estimated by the Board for March, 1921, as 51–60 per cent, while the Bureau's estimate for December, 1920, is 38 per cent and for May, 1921, 44.2. In the absence of detailed statements of the methods employed in collecting the material, no explanation of these differences can be made. The method employed by the Board is to send questionnaires to "real estate boards, brokers, chambers of commerce, social organizations and individuals." The exact form of the questionnaire is not revealed, but from the miscellaneous character of the sources of information it may be presumed that an estimate of the amount of change is requested. The answers to such a questionnaire probably exaggerate the changes in rents. If a real estate broker is asked how much rents have risen or fallen in a given period, he is likely to place too much weight on the changes which have come to his attention and insufficient weight on the cases in which no changes have occurred. If the Bureau of Labor makes an actual study of the books of real estate agents, its figures for changes in rent are probably better than those of the Board.

There are two important differences in the methods used by the Board and the Bureau in making their index numbers for the cost of clothing. In the first place, the Bureau averages changes in the prices of various articles of clothing to find the change in the cost of clothing for each city, and then averages the city index numbers to find an index number for the United States as a whole, while the Board averages directly the returns from a number of dealers in different cities. The Board draws its quotations from a larger number of cities. Secondly, and a more likely cause of the disagreement, is the difference in the systems of weights. The Bureau's index number for clothing is unweighted, while that of the Board is weighted on the basis of a clothing budget. In the absence of the detailed price figures used by

the Bureau, it is impossible to establish the effect of this difference, but the presumption is strongly in favor of a weighted index number of clothing.

As has already been noted, the group of expenditures which is called sundries by the Board is divided by the Bureau into two groups known respectively as miscellaneous and furniture and furnishings. The index number for sundries is made by the Board from prices of "car fare, doctor's fees, church contributions, organization dues, insurance, reading, amusements, candy, soft drinks, tobacco, furniture and furnishings, and household supplies." But only rough estimates of changes in the prices of these articles are made, and the weight assigned to each of these articles or groups of articles is not stated. The Bureau of Labor Statistics does not publish the list of articles included in the miscellaneous and furniture and furnishings groups, nor the system of weights employed. Even conjecture as to the cause of the disagreement is, therefore, impossible.

In what has been said above, attention has been directed to the disagreement of the two index numbers. From a wider point of view, their correspondence is more important. For many years the question of devising a more adequate measure of the cost of living than the index number of retail food prices has been discussed, and the opinion has frequently been expressed that the difficulties to be faced were insuperable. When differences in material and method are taken into account, the substantial correspondence of the two index numbers here compared is the best proof that such a measure is practicable. Neither the index number of the Board nor that of the Bureau is the last word in measures of the cost of living, but they possess the great merit of having demonstrated the practicability of such measures. The index number of the Board has contributed materially to establishing the influence of measures of change in the cost of living in wage determinations. The more frequent appearance of the Board's index number has facilitated the use of such measures of change, and its general correspondence with the index number of the Bureau has greatly enhanced the prestige of such measures. The Board has done a useful piece of work and it is to be hoped that the maintenance of an index number of the cost of living will be a permanent part of its work.

ON THE BEST FORM OF INDEX NUMBER

By Royal Meeker, *International Labor Office, Geneva*

There is undoubtedly at the present time a keener interest in price fluctuations and a better understanding of index numbers as a measure

of such fluctuations than ever before. The importance of measuring price changes accurately and promptly makes the consideration of the methods employed in constructing index numbers especially timely. It is a matter of keen regret to me that distance and the duties of my new office made it impossible for me to attend the eighty-second annual meeting of the American Statistical Association, and to hear Professor Irving Fisher's views on the best form of index number and the illuminating discussion which followed. It is not clear from the brief summary of Professor Fisher's paper published in the March issue of the QUARTERLY whether he considers the mathematical formula for computing an index number of equal, greater, or less importance than the other characteristics of index numbers which he mentions. My own experience in recalculating the wholesale and retail price indices of the United States Bureau of Labor Statistics convinced me that the particular mathematical formula to be used in computing index numbers, while not a trifling matter, is of minor importance compared with the commodities to be selected for inclusion in the index, their prices and their quantities to be used as weights. Although the range of my experimentation was rather limited, it did not seem to me that it made much difference what machinery of calculation was used as long as the raw materials from which the index numbers are made is of good, sound, dependable quality. To borrow from Professor Fisher's picturesque and forcefully descriptive vocabulary, "it matters but little what mathematical formula we use in computing the indices of p's and q's as long as we mind our p's and q's." If Professor Fisher or any one else had convinced me in 1914 of the superiority of the geometric mean over the weighted arithmetic mean or "price aggregate" method of computing index numbers of wholesale prices, I would cheerfully have discarded the arithmetic mean and adopted the geometric mean. I would have done so notwithstanding the fact that a "price aggregate" is easily comprehensible by any one of ordinary intelligence as the cost of a given bill of goods, whereas the geometric mean is understandable only by those who have a pretty clear comprehension of mathematics.

I am quite puzzled by the rather bizarre mathematical formula employed by Professor Fisher to illustrate the defects of the weighted arithmetic mean. Certainly no one advocates using such a double system of weighting today. The compilers of the old index numbers published by the British Board of Trade made use of weighted price relatives, but these price relatives were weighted by constants representing the estimated money values of each commodity in the base year or period. The mathematical method employed in computing the old Board of Trade index numbers was sound enough. The defects in these

indices were to be found in just those things which are left out of Professor Fisher's discussion, viz., the character and number of the commodities included, the sources of the "prices," and the quantities used as weights. I do not wish to be understood as undervaluing mathematical precision in calculating index numbers, but I feel sure that even the absurd double weighted mathematical formula given in the illustration by Professor Fisher could be applied to any series of index numbers which includes all important representative commodities without very much changing them. My experience in recalculating the Bureau of Labor index numbers of wholesale prices convinces me that this would prove to be the case. What could be more absurd than the wildly erratic weightings contained in the so-called "unweighted" index number, in which the prices of nutmegs, pig iron, wheat, 1-inch augurs, cotton, quinine, potatoes, chisels, and pork all had equal influence upon the general level of prices? Certainly it was natural that I should feel qualms of misgiving during the process of transforming the old "unweighted" index numbers into the new "weighted" ones, especially as I was obliged to do some most heroic "estimating" in arriving at some of the quantity weights to be used. I shall deal with the problems of quantity weights later. Yet the discrepancies between the old Bureau of Labor index and the new are not very great, not nearly so great as I expected. Professor Mitchell, who advised and assisted me throughout in the recalculation, was also astonished, I think, that greater differences were not shown. In fact the differences were so slight that I was led to remark that no matter what the statistician does with an index number, he is always bound to come out right. This seems scarcely an exaggeration as long as the index contains all the important representative commodities and the statistician is honest and does not purposely try to make the index "point a moral or adorn a tale."

It seems to me that Professor Fisher is trying to perform a miracle, or at least to accomplish the impossible. Indices, like all human devices, are strictly limited in their uses and applications. While I was commissioner of labor statistics, I received communications beseeching me, as official price compiler and controller, to use my authority to bring prices down. Some persons apparently thought that prices were rising because the index numbers were increasing, and that all that was necessary to bring down prices was to decrease the index number. How very fortunate for the compilers of index numbers that prices cannot be controlled by index numbers! It is, perhaps, not so fortunate that index numbers are so limited in their usefulness. A price index number can never show anything but variations in prices, and even

that only imperfectly. I suppose Professor Fisher's statement that index numbers are "precise within much less than 1 per cent" refers to the precision of the mathematical computations. I think the raw materials from which price indices are made contain much more than 1 per cent of impurities. A price index cannot be made to show variations in the quantities of commodities produced, exchanged, or consumed. Nor can an index number of quantities be made to show variations in prices. Yet, if I understand Professor Fisher, he would perform the miracle of showing by one and the same index variations in production (or exchange or consumption) and variations in prices. It does not seem to me that he has met Professor Persons' criticism. I do not doubt for a moment that Professor Fisher's formula will stand on its head and do all the other things he says it will do. I am not at all certain, however, that an index number that can stand on its head is to be preferred above all other indices on that account. Professor Fisher's formula would give us the square root of the product of an index number, say for 1920, calculated by the price aggregate method on the quantities of goods purchased in 1920 multiplied into a price index for 1920 calculated by the same method on the quantities of goods purchased, say in 1915. We have here an inextricable mixture of changes in prices and changes in quantities of goods. What does this "ideal index," which has been achieved by so much mathematical labor, represent? Is it an index of price changes? Or is it an index of quantity changes? The answer seems clear that it is both and neither. The perfect reversibility of the index number does not seem to me to be of any consequence at all. The all important fact is that this number does not mean anything that can be clearly grasped. Judging by my experience with index numbers, it seems to me more than likely that the quantity obtained by Professor Fisher's formula would differ but slightly from year to year, or even over five or ten year periods, from a price index computed by the formula I used for calculating the present wholesale price index of the Bureau of Labor Statistics. This fact, if it be a fact, is also of little consequence, for even if the index computed by Professor Fisher's formula differed from the Bureau's index as widely as the East from the West, the former index would still be as imcomprehensible as ever.

It is a source of some satisfaction to me to note that Mr. Walsh, at the close of his discussion, advises for practical purposes the computation of index numbers by the same mathematical formula used by me in calculating the present weighted index numbers of the Bureau of Labor Statistics. The source of my satisfaction comes not so much from the agreement upon the formula as from the fact that Mr. Walsh realizes

the practical difficulty of securing acceptable quantity weights, at least for past years. It seems to me that the perfect reversibility and the double weighting by the quantities in the base year and in the year for which the index is calculated, upon which Professor Fisher lays so much stress, are of no value at all; they may even constitute positive demerits. To insist upon these characteristics for the best index number is even more irrelevant than to insist that the best watch dog must be able to walk on his hind legs. Walking on his hind legs has nothing to do with his watchfulness, and therefore it need not detract from the dog's usefulness as a watch dog; but the double weighting proposed by Professor Fisher does affect the usefulness of the index as a measure of changes in either prices or quantities of goods.

I assume that Professor Fisher desires to have the price statisticians obtain the quantities of commodities each year. This would be impracticable or even impossible. Reliable estimates of quantities can be obtained for the basic raw materials and semi-manufactured goods each year; but for manufactured commodities, clothing, metals, manufactures, most building materials, and house-furnishing goods, the figures are not very accurate even in the census years, and it would be very difficult if not impossible to obtain trustworthy figures each year. It may be argued that very rough estimates of quantities are sufficiently accurate for weighting purposes and that the guesses and estimates from manufacturers and jobbers would be satisfactory. If this be true, it is an argument against going through the toil, trouble, and expense of gathering this very imperfect information and using it in the computation of index numbers by any formula. If quantity weights are to be used the quantities should be approximately accurate. To be sure, under the impulse of war-time patriotic fervor and strong pressure from the government, business firms did report their production quite accurately to the War Industries Board. But a War Industries Board with all its cumbersome and expensive machinery could not be maintained in time of peace. I see no hope of getting each year quantity weights of sufficient accuracy to make it worth while to use them in Professor Fisher's formula. Even if satisfactory quantities could be obtained, the labor of performing the required mathematical computations would be so considerable that I scarcely think public statistical offices would undertake it.

Mr. Flux, the compiler of the new wholesale index number of the British Board of Trade, regards accuracy in the quantity weights as of minor importance in an index computed by the method of the geometric mean. He therefore used as weights rough estimates of the value of goods produced and imported into the United Kingdom as

shown in the census of production of 1907, the latest figures available. Mr. Flux expressed the opinion, however, that accuracy in weights is essential in an index computed by the method of the arithmetic mean, which is the method used in computing the United States Bureau of Labor Statistics index and which is recommended by Professor Fisher for the proposed index in all calculations up to the final one. It would surely seem that if quantity weights are to be used at all they should be as accurate as possible. I therefore think it would be quite sufficient to revise the quantity weights every ten years, as soon as the decennial census figures become available.

Even more important than accuracy in the quantity weights are accuracy and representativeness in the price quotations. The prices used by the Bureau of Labor Statistics may be criticised on the ground that they are not homogeneous since they are obtained from different sources. Of 371 quotations in 1919, 203 are obtained from trade journals, 152 from manufacturers or sales agents, 13 from boards of trade, and 3 from federal or state bureaus. While commissioner of labor statistics, I tried to secure all quotations direct from jobbers and wholesalers, but with the slender resources of the Bureau it was impossible to discontinue the use of the trade journals. All prices were thoroughly checked and revised, however, and it can be said with confidence that they are the best obtainable unless much more money is to be spent than has ever been spent on wholesale prices by the Bureau. It may be worth stating, in view of the charges of extravagance in government expenditures, that only two employees working only part of the time on wholesale prices have kept this work up to date, so that the indices for the nine groups of commodities and for all commodities are issued to the press, and to all who desire them, each month for the preceding month. It is very desirable that more money should be spent in securing absolutely trustworthy *wholesale* prices as distinct from either *manufacturers'* prices or *trade journal* prices. The Federal Trade Commission, the Federal Reserve Board, or some other federal agency should take over the task of gathering these price statistics. It is important that *manufacturers'* or *producers'* prices should be gathered as well as *wholesale* prices and that index numbers be computed for both series of prices.

The wholesale prices of the Bureau of Labor Statistics are open to criticism in that too many quotations are secured from the big cities. Of 32 quotations of farm products, 19 are obtained from Chicago; of 100 food products, 26 are from Chicago and 42 from New York; of 21 fuel and light, 9 are from New York; of 43 metals and metal products, 20 are from New York and 12 from Pittsburgh; of 32 lumber and building material,

23 are from New York; of 18 chemicals, all are from New York; of 23 miscellaneous, 12 are from New York; of a total of 371 quotations, 56 are from Chicago and 132 from New York. Of course New York and Chicago are the largest markets in the country, and the trade journals and selling agencies are concentrated in these great centers; but it is obvious that prices to be representative should be scattered more widely. This means expending more money than Congress appropriates to the Bureau of Labor Statistics.

In my view the best way to achieve the "best index number" is first, to secure more trustworthy and more representative prices from (1) *producers* and (2) *jobbers* and *wholesalers*, and secondly, to obtain more accurate statistics and estimates of quantities of goods produced, imported, exported, and consumed. It is especially necessary that the estimates made by me in 1914 of the quantities of goods of various kinds entering into *exchange* be carefully checked and revised in the light of more recent and more detailed information. It is desirable that several different kinds of index numbers be prepared by the federal agency having in charge the collection of producers', jobbers', and wholesalers' prices and the compilation therefrom of price indexes. Much more time and effort should be devoted to the subject of producers' and wholesale prices than has been devoted to them in the past. When the quantities and the prices are thoroughly satisfactory I think it will make but little difference what mathematical formula is used in calculating the index numbers.

THE BUSINESS INDICES OF THE *FRANKFURTER ZEITUNG*

It has long since been the policy of the *Frankfurter Zeitung* to publish original statistics with regard to economic conditions in Germany. These statistics are of particular interest at the present time because of the chaotic condition of the German credit and currency situation.

The most interesting of the statistical series which are now compiled and published by this journal are its index numbers on wholesale commodity prices, securities' prices, wages per hour in Frankfurt, and foreign exchange values, its compilations of bankruptcies, and its compilation of securities issued in Germany. All these statistics appear in the current numbers of the paper. The index of securities prices is published weekly; the wholesale price index and the statement of securities issued appear at the first of each month; the bankruptcy figures are published quarterly, while the figures on wages have appeared only once (in the second edition of May 22, 1921), and the foreign exchange figures only once (on February 26, 1921). The index numbers among these series have also been published from time to time within the last six months in pamphlet form called, in its first edition, "Die Indexziffern der Frankfurter Zeitung," and thereafter "Die

Indexzahlen der Frankfurter Zeitung." Each edition bears upon its cover the name of Ernst Kahn, and opens with an introduction by him.

The method by which these indices are constructed is very simple. In each case the index represents an unweighted arithmetic average of relatives, using quotations as of January 1, 1920, as a base. In introducing the wholesale price index number, the author says:

"Unfortunately up to this time Germany has lacked a good wholesale price index. We hereby attempt to give one. In the construction of the commodity price index of the *Frankfurter Zeitung*, the investigations which Professor Irving Fisher sets forth in his two great books, 'The Purchasing Power of Money' (which was published in German by G. Reimer [Berlin] in 1916) and his newest work 'Stabilizing the Dollar' have been taken into account along with our own information on the subject. The layman will demand as the first requisite for a good commodity index number that in the assembling of the quotations each commodity shall be valued or weighted according to its importance in industry. Fisher's investigations show that this is not necessary. The following graph taken from Fisher's work makes that evident; we see that computation with or without the use of quantity weights gives parallel curves." (At this point the graph, which appears on page 3 of "Stabilizing the Dollar," is reproduced.) "It would take us too far afield to set forth here the theoretical grounds of this noteworthy discovery, but these investigations of Fisher's have given us the courage to attempt to carry out our long cherished desire to compute a commodity index number. The difficult question of placing the individual commodities according to their importance had always kept us, before this, from undertaking the task."

The commodity index of the *Frankfurter Zeitung* is, however, roughly weighted by the choice of commodities included. In the index as it now stands* there are seven quotations for chemicals, three quotations for coal, and five for iron and steel products. Raw cotton, cotton yarn, and cotton cloth are all represented, as are hides, leather and shoes; flax and linen yarn are both included, and raw jute and jute bagging, but wool and silk are represented by only one quotation apiece.

This index contains 77 commodities, divided into four groups as follows:

Group I. Wheat, rye, oats, barley, corn, potatoes, beans, peas, lentils, rice, eggs, meat, lard, margarine, butter, milk, condensed milk, powdered cocoa, coffee, sugar, beer, tobacco, hay, straw, hops, wine.

Group II. Raw cotton, scoured South-German sheep's wool, silk yarn (organzine), cotton yarn, cotton cloth, artificial silk, flax, linen yarn, raw jute, jute bagging, twine, hides, leather, shoes, rubber.

Group III. Non-coking bituminous coal, coking coal, foundry coke, Rhenish lignite, machine pressed peat, iron ore, pig iron, rolled plates, steel scrap, iron bars, copper, tin, zinc, lead, aluminium, nickel, silver, gold.

Group IV. Machine oil (refined), petroleum, benzine, benzol, newsprint paper, cement, bricks, salts of potash, sulphate of ammonia, lime nitrogen, kainite, brown leather dye, alcohol, timber, firewood, calcium carbide, saltpetre, coal tar oil.

The "peace index" (*i. e.*, the index number applying to the middle of 1914) and that for January 1, 1920, are the only ones, however, which include all the com-

* See "Die Indexzahlen der Frankfurter Zeitung," 4th edition, dated May 17, 1921.

modities noted. In every other month it has been impossible to get a complete list of quotations. It is, in fact, remarkable, considering the chaotic state of German prices, that omissions have not been more numerous.

WHOLESALE PRICE INDEX OF THE *FRANKFURTER ZEITUNG*

	Foods, etc.	Textiles, leather, etc.	Minerals	Miscellaneous	All commodities
Peace index	11.38	3.67	7.15	12.0	9.23
1920					
January 1	100	100	100	100	100
February 1	119	153	145	123	131
March 1	126	165	163	147	147
April 1	135	151	135	170	146
May 1	134	119	133	170	156
June 1	128	98	110	197	134
July 1	146	94	111	195	136
August 1	149	97	112	194	141
September 1	150	103	118	193	144
October 1	153	108	124	191	146
November 1	165	111	120	199	152
December 1	177	99	117	195	153
1921					
January 8	154	97	120	189	148
February 5	144	92	113	183	136
March 5	137	87	111	182	131
April 2	136	79	115	180	130.1
May 7	133	78	112	194	131.8
June 4	127	70	109	196	128
July 2	144.5	79	115.3	190.6	135.4

The statistics used in the index number of wages per hour paid in Frankfurt represent 23 different trades. It was possible, however, to obtain comparable rates in 1914 for only 9 of these trades, so that the index number uses January 1, 1920, as a base, and gives no figures for 1914. It is computed by averaging the monthly relatives obtained for each industry. The figures do not take into account unemployment and hours of labor in the various industries represented and do not, therefore, give any indication of the actual earnings of wage-workers in Frankfurt.

The following table presents the wage index of the *Frankfurter Zeitung:*

FRANKFURT-AM-MAIN

	Index number of hourly wages
1920	
First of January	100
February	112
March	122
April	146
May	168
June	169
July	184
August	186
September	186
October	188
November	198
December	200
1921	
Beginning of the year	203
First of May	219

The following table gives the index numbers for wages in the industries for which 1914 rates could be obtained:

RELATIVE WAGES PER HOUR IN FRANKFURT-AM-MAIN*
(JANUARY 1, 1920 = 100)

Industry	1914	January 1, 1920	July 1, 1920	Beginning of 1921	May 1, 1921
Building	22	100	195	226	233
Metallurgical	22	100	206	230	230
Soap	16	100	190	208	221
Food	16	100	169	211	257
Shoe	22	100	157	174	222
Decorating	22	100	203	242	265
Rubber	21	100	238	255	272
Chemical	22	100	250	279	291
Mining	17	100	159	180	200

* In the majority of these cases the increase in wages seems to have been approximately the same as the increase in the cost of living. According to the cost of living index for Frankfurt compiled by Dr. Elsas, living costs in that city have risen from 26.5 on January 1, 1914, to 280 in May, 1921 (using April 1, 1919, as 100). In dealing with these figures, it should be remembered that living costs have been kept as low as they are by the government regulation of rents and of the prices of some foodstuffs.

It seems regrettable that none of the index numbers of the *Frankfurter Zeitung* is weighted. It is particularly unfortunate in the case of the index of foreign exchange values, where an unweighted arithmetic average is especially unreliable. In this case, however, it would be particularly difficult, as Herr Kahn points out, to obtain the data necessary for the compilation of weights, since the most recent statistics giving German trade by countries apply to the period from January through August, 1920. He therefore heads his paragraphs explaining this index, "Der Versuch eines Valutenindex" ("A Tentative Foreign Exchange Index"). The quotations included are those on eleven countries: the United States, England, France, Holland, Switzerland, Sweden (as the greatest of the Scandinavian countries), Italy, Spain, Belgium, German-Austria (as the representative of the former Austro-Hungarian territory), and Roumania (as the country in the Balkans of the greatest importance to Germany at the present time). The following table gives the figures arrived at by averaging the relatives of quotations on these countries, using the par value of the mark as 100:

GERMAN FOREIGN EXCHANGE INDEX
(Par value of the mark = 100)

1920	
January	13.4
April	10.1
May	7.3
June	18.3
July	18.2
August	17.2
September	15.5
October	13.1
November	10.5
December	11.7
1921	
January	11.1
February	11.6
March	12.2
April	11.9
May	10.8

The index number of securities prices (the Bourse index) contains quotations of 25 shares and 10 bonds. Its fluctuations are summarized in the following table:

INDEX NUMBERS OF SECURITIES PRICES

	10 Bonds	25 Shares	Total
1919			
September 1	97	79	87
1920			
January 1	100	100	100
February 1	120	139	*137
March 1	145	*158	*157
May 3	114	*135	133
June 1	105	110	109
July 1	106	119	117
August 2	106	144	140
September 4	*122	152	*148
October 2	122	*165	160
November 6	143	*181	177
December 4	136	184	178
1921			
January 8	132	*202	194
February 5	121	165	159
March 5	118	*158	153
April 2	117	169	163
May 7	120.5	*166	*161
June 4	120.6	172	166
July 2	125	188	181

* Corrected in accordance with the aggregate prices published by the *Frankfurter Zeitung*. In these cases the index number published does not agree with the corresponding absolute figures. The deviations are in most cases very slight.

FAITH M. WILLIAMS

Federal Reserve Board

ON PROFESSOR MORITZ' METHOD FOR COMPUTING FREQUENCY PERCENTAGES

In a recent article* Professor Moritz sets forth a new method for computing frequency percentages. The same method may be applied in computing the percentages that the frequencies are of N. In certain psychological experiments each of N individuals attempts all of a number of problems, and the frequencies of success or failure for each problem are

$$f_1, f_2, f_3, \quad . \quad . \quad . \quad , f_n.$$

It is required to compute the percentages

$$P_k = 100 f_k/N, \; k = 1, 2, 3, \quad . \quad . \quad . \quad , n.$$

The two steps in the method analogous to those of Professor Moritz will be:

(1) Replace the frequencies f_k by proportional numbers f'_k, the factor of proportionality being N'/N, where N' approximates 100.

(2) Distribute the difference $D = 100 - N'$ over the numbers f'_k in accordance with the formula $\Delta f'_k = f'_k \, . \, D/100$, where $\Delta f'_k$ is that portion of D which falls to f'_k.

* "On a New Method of Computing Frequency Percentages," QUARTERLY PUBLICATIONS OF AMERICAN STATISTICAL ASSOCIATION, March, 1921.

Since $N'+D=100$ it follows that

$$P_k=f_k\frac{(N'+D)}{N}.$$

The approximation P'_k is obtained by multiplying the small quantity D/N by $N'/100$, the last ratio approximating unity. Thus,

$$P'_k=f_k\left(N'/N+\frac{DN'}{100N}\right) \text{ or}$$

$$P'_k=f'_k+\Delta f'_k.$$

It may be noted that the error

$$P_k-P'_k=f'_k\frac{(100-N')^2}{100N'}.$$

The relative error is given by the expression

$$R=\left[\frac{100-N'}{100}\right]^2.$$

The following problem illustrates the method:

FREQUENCY AND PER CENT FAILING

k	f_k	f'_k	$\Delta f'_k$	P'_k
1.....	4	2.4	−.02	2.38
2.....	7	4.2	.03	4.17
3.....	15	9.0	.07	8.93
4.....	28	16.8	.13	16.67
5.....	35	21.0	.17	20.83
6.....	43	25.8	.21	25.59
7.....	64	38.4	.31	38.09
		117.6+−.94=116.66 (check)		

Suppose $N=168$. Choose $N'/N=.6$ so that $N'=100.8$. (1) $f'_k=.6f_k$. (2) $100-N'=-.8$, $\Delta f'_k=-.8\%$ of f'_k.

It may be noted that the advantages of the above method are limited unless N'/N is chosen so as to give a simple ratio. In the above example all of the calculations may be done mentally.

KARL J. HOLZINGER

ANALYTICAL SUMMARY OF THE NEW ZEALAND CENSUS OF 1916*

In a recently issued report the principal facts of the New Zealand Census of 1916 are summarized and analyzed. The tabular parts of the census have already

* *Report on the Results of a Census of the Population of the Dominion of New Zealand taken for the Night of the 15th October*, 1916. Malcolm Fraser, Government Statistician, Wellington. By authority: Marcus F. Marks, Government Printing, 1920.

been issued in separate bulletins. Among the features of this volume are a historical sketch of New Zealand census takings and a study of family statistics, including conjugal condition and fertility, dwellings and households.

The first general census of New Zealand took place in December, 1851, although since 1841 annual returns of population for the various settlements had been made by local resident magistrates. Since that date censuses have been taken at least as often as twice in a decade, and in the sixties and seventies three times in a decade. The census enumeration has always been of the de facto population.

With regard to the chapters on conjugal condition and fertility, an interesting feature is the combination of the results of the census with those of the registration statistics of births. Among others, tables are presented showing relative ages of husbands and wives at the date of the census, and annual birth-rates for each combination of ages; ages of wives in connection with the number of issues born and the number living; average number of children to married women of different ages; percentages of married women of different ages who at the date of the census were childless or had borne one or more children; and the average number of children to marriages of different durations.

The section on dwellings and households contains interesting material. Households are classified by sex and marital condition of the head of the household, by number of breadwinners, by number of non-breadwinners, by number of children under 14 years of age; the principal findings have already been discussed in a previous note.

Besides these subjects chapters are devoted to the findings with regard to age, birthplace, length of residence in New Zealand, religion, education, infirmity, and occupation; a special chapter is devoted to a study of race aliens, and appendices discuss the censuses of Maoris and of the outlying islands.

ROBERT M. WOODBURY

MISCELLANEOUS NOTES

Circulation Inquiry on the Monthly Review of the Federal Reserve Bank of New York. The Federal Reserve Bank of New York has just completed a tabulation of the returns from a circular letter sent to the persons on the mailing list for its *Monthly Review of Credit and Business Conditions*. The *Review* has had a circulation of about 50,000. More than 15,000 copies are sent to member banks, but the remainder are sent to a mailing list secured in a somewhat wholesale fashion by a directory company. The recent circular letter was sent to 34,000 persons and asked whether they wished to continue to receive the publication and also what features of the report the readers found most interesting. Replies were received from 17,000 persons, of whom 90 per cent wished to continue to receive the *Review*. The negative replies were largely due to retirement. The following table shows the percentage of the persons replying affirmatively who mentioned different items as interesting.

Articles	*Percentage*
Credit Conditions	60.4
Prices	48.2
Wages	46.6
Securities	45.9
Banking	43.6
Money Market	42.4
Cost of Living	39.9
Employment	37.4
Production	33.8
Foreign Trade	33.7
Federal Reserve Banks	28.3
Retail Trade	27.7
Building	23.8
Particular Industries	10.7
Other	6.6

As was to be expected, the choice of persons in different professions varied considerably. Merchants and manufacturers were most interested in the general survey of credit conditions. College professors, statisticians, and other professional persons were most interested in prices. Bankers were naturally most interested in banking. Lawyers were most interested in securities. Manufacturers gave their second vote to wages. A great many of those who sent answers made special comment on one feature or another of the *Review*. The feature most frequently mentioned was the diagrams which are included in the *Review*.

The National Bureau of Economic Research. During the past twelve months, the National Bureau of Economic Research has been engaged in estimating the income of the people of the United States for each of the years since 1909. The director of research, Wesley C. Mitchell, has been assisted by three staff members, each working on a separate phase of the inquiry. Oswald W. Knauth has been approximating the total income by summating the amounts reported to the income tax authorities and the estimated income falling outside the taxable field. Willford I. King has been attempting to reach the same goal by estimating the value product of each of the important branches of industry, using the census as the principal source of material. Frederick R. Macaulay has been working on the problem of ascertaining the approximate number of income recipients falling into each income class. It is expected that preliminary estimates of totals will be in the hands of the directors of the Bureau in the near future, and that the analysis for the last decade will be ready for publication late in 1921. After the completion of this study, it is planned to investigate the causes and mechanism of business cycles. The Bureau expects to coöperate with other scientific workers interested in the same line of research. As the Bureau will confine itself to the field of description and explanation, it will not compete with organizations devoted to forecasting.

Industrial Relations Conference. An Industrial Relations Conference, authorized by Governor William C. Sproul of Pennsylvania, is being arranged by Dr. Clifford B. Connelley, Commissioner of the Department of Labor and Industry, Commonwealth of Pennsylvania, to be held at Harrisburg, Pa., October 24 to 27. This conference will include the features of the Welfare and Effi-

ciency Conferences and Safety Congresses of other years. The main topics to be discussed will include industrial waste, industrial coöperation, industrial publicity, industrial education, women and children in industry, and the medical supervision of industry.

Conferences on Statistical Subjects. The Department of Statistics of the Federal Reserve Bank of New York has been holding a series of weekly conferences on statistical subjects. Among the speakers were Dr. H. Parker Willis, Federal Reserve Board; Professor Irving Fisher, Yale University; Dr. Willford I. King, Bureau of Economic Research; Governor Benjamin Strong, Federal Reserve Bank of New York; and Dr. Wesley C. Mitchell, New School for Social Research. The conferences have been suspended for the summer, but they will be resumed in the fall. These meetings are open to statistical workers in New York City.

A Statistical Bureau Suggested for the British Empire. At the conference of Prime Ministers of the British Empire, which was held during June in London, the question of the establishment of a British Empire Statistical Bureau was under discussion, on report by the Statistical Conference held in 1920. An Inter-departmental Committee on Statistics for the United Kingdom has recently presented a report on the Organization of Official Statistics in Great Britain.

Committee on Nomination of Officers for 1922. In accordance with Article IX of the Constitution, the president has appointed a committee to nominate officers for election at the annual meeting, to be held in Pittsburgh in December next. Twelve or more regular members or fellows may make nominations by submitting signed petitions to the nominating committee at any time after the appointment of the committee and up to the time of the annual meeting.

The committee is composed of Walter F. Willcox, Cornell University, chairman; Charles E. Baldwin, United States Bureau of Labor Statistics, Washington, D. C.; and F. Leslie Hayford, General Motors Corporation, New York City.

Annual Meeting of the Statistical Association. The Association will hold its annual meeting in Pittsburgh on December 27–29 inclusive. The American Economic Association meets at the same time and place. Suggestions with reference to this meeting addressed to the secretary of the association will be placed in the hands of the Program Committee. Program announcements will be mailed to each member.

Special Meeting of the Association. The Committee on Business Research held a meeting on June 17 at the Machinery Club in New York City. Charles C. Parlin, head of the Department of Commercial Research of the Curtis Publishing Company, gave the principal address, on the topic, "Commercial Research and Business Policies." Keen interest was shown in the subject, as was indicated by the attendance of about 120 members and their guests, and by the general discussion which followed the main paper.

Dr. Joseph A. Hill has been made assistant director of the census; his appointment was confirmed by the Senate on July 13, 1921.

Dr. W. Randolph Burgess, who was associated with Colonel Leonard P. Ayres in the Statistics Branch of the General Staff of the Army, and in the statistical work of the Russell Sage Foundation, is now with the Statistics Department of the Federal Reserve Bank of New York as chief of the Reports Division. He has in charge the editing of the *Monthly Review of Credit and Business Conditions.*

Mr. G. H. Knibbs, commonwealth statistician of Australia, has been appointed director of the Australian Bureau of Science and Industry.

Mr. G. Findlay Shirras, director of statistics of India, has been appointed director of the new Labour Office, in the Secretariat, Bombay, India.

Dr. John J. Tigert, formerly professor of psychology in the University of Kentucky, has been appointed United States commissioner of education.

The Federal Reserve Bank of New York has taken on for special summer work in the Department of Statistics instructors and graduate students from a number of colleges and universities. The list includes the following names:

M. J. Williams	Harvard Graduate School of Business
B. B. Smith	" " " " "
I. G. Richardson	" " " " "
C. Rogeberg	" " " " "
E. R. Lilley	New York University
F. T. Taylor	Tuck School, Dartmouth College
J. D. Brown	Princeton University
C. A. Warburton	Cornell University
Muriel Bowden	Brearley School, New York City
Clara Eliot	Columbia University

Mr. F. B. Rankin, Mr. Samuel Michener, and Miss Minetta Goetz, formerly students in the statistical and actuarial courses at the University of Michigan, are now employed in the Statistical Department of the American Telephone and Telegraph Company in New York City.

NEW MEMBERS ELECTED SINCE MAY 1

Duncan, C. S., consulting economist, Southern Wholesale Grocers Association, New York City

Ellis, George G., statistician, Public Service Dept., Merchants National Bank, Los Angeles, Calif.

Gebhart, John C., director, Dept. of Social Welfare, A. I. C. P., 105 E. 22nd St., New York City

George, E. E., Valuation Division, City Transit Office, Philadelphia, Pa.

Gunnarson, Arthur B., School of Business, University of Minnesota, Minneapolis, Minn.

Holzinger, Karl J., School of Education, University of Chicago, Chicago, Ill.

Kile, Kathryn L., Cost and Circulation Analyses, Outer's Book Co., Chicago, Ill.
Kolhert, Edward O., industrial engineer, Edison Storage Battery Co., Orange, N. J.
Laidlaw, Walter, research and fellowship secretary, New York Federation of Churches, 200 Fifth Ave., New York City
McEwen, George F., Scripps Institution for Biological Research of the University of California, La Jolla, Calif.
McKenney, William F., U. S. Geological Survey, Washington, D. C.
McKernan, John F., Western Electric Co., 195 Broadway, New York City
May, Grace I., Corporation Buying Dept., Hakey Stuart & Co., Chicago, Ill.
Mills, Frederick C., School of Business, Columbia University, New York City
Milne, Frank D., American Tel. & Tel. Co., 195 Broadway, New York City
Nicholson, S. L., asst. to the vice president, Westinghouse Electric and Manufacturing Co., East Pittsburgh, Pa.
Peirce, Mary F., International Accountants Society, Chicago, Ill.
Piper, G. D., asst. auditor, Westinghouse Electric and Manufacturing Co., East Pittsburgh, Pa.
Ponce de Leon, Jose M., census inspector for the Mexican Federal Government, Chihuahua, Mexico
Rehfuss, John F., Federal Reserve Bank, Philadelphia, Pa.
Sealy, Maria P., Planning Dept., R. H. Macy & Co., New York City
Thorp, Willard L., Dept. of Economics, Amherst College, Amherst, Mass.
Weeks, Estella T., Research Dept., Hoggson Bros., 485 Fifth Ave., New York City
Widdemer, Kenneth D., American Red Cross, 119 W. 40th St., New York City
Willis, H. Parker, director, division of Analysis & Research, Federal Reserve Bank, New York City

REVIEWS

Principes de Statistique Theorique et Appliquée. By Armand Julin. Paris: Albert Dewit. 1921. xxiii, 712 pp.

This work on statistical method is more complete than any similar treatise in English. It begins with a review of the development of the science and then proceeds with the discussion of the principles involved in the usages of present day statisticians. Little effort is made by M. Julin to develop new methods, but he not only attempts but succeeds most admirably in explaining the philosophy underlying the principal processes utilized by statisticians. He compares and criticizes the methods that they follow, and makes clear his reasons for selecting those modes of procedure which he believes to be best adapted to specific classes of problems. In this criticism he shows marked familiarity with the writings of both English and American statisticians.

The book is so voluminous that it seems better adapted for service as a general reference work or as a guide for advanced students than as a text for beginners.

Most of the formulae developed are elementary in nature, hence the non-mathematical student of statistics can understand the majority of them without difficulty.

In general, the author follows the customary terminology, but in a few instances he introduces innovations. For example, he uses the term *covariation* instead of *correlation* to signify any similarity in the fluctuations of two variables. He advocates this procedure because it makes no assumption that the two variables are in any way dependent upon each other, an implication which many statisticians believe to be inherent in the word *correlation*. Despite the admirable desire for accuracy evinced in this respect, it seems to the reviewer that M. Julin has failed to distinguish clearly between the two related but entirely dissimilar ideas represented respectively by the coefficient of correlation and the ratio of variation. It must be said, however, that in failing to emphasize this distinction he is by no means alone.

Although oversights occasionally occur, they are so rare that they stand out as contrasts to the accuracy characterizing the work as a whole. The reasoning is lucid and logical and the style is both simple and pleasing.

This book is decidedly more complete than most similar studies and compares favorably in clarity and definiteness with any of the previously existing works on statistics that have come to the attention of the reviewer.

WILLFORD I. KING

Enquiry into Production. International Labour Office. London: Harrison & Sons. 1920. 188 pp.

This Introductory Memorandum is a remarkable publication. Unfortunately, its extraordinary qualities lie in its defects. It is inadequate, unintelligent, un-

discriminating, at points even slovenly. Its general deficiency can only be appreciated from first-hand acquaintance; a brief review cannot hope to convey a clear impression of the publication's offences. But some account of the Memorandum seems desirable, especially since its appearance raises serious questions concerning the future work of the International Labour Office.

The Memorandum opens with a report of the discussion which led the Office to undertake the *Enquiry into Production*. Apparently the investigation had its origin in a consideration of the effects of the eight-hour day upon the volume of production. Discussion of this specific issue gradually widened to include all factors contributing to the "crisis of underproduction." Finally, at its Genoa meeting in June, 1920, the Governing Board of the Office ordered "an enquiry into industrial production throughout the world, considered in relation to conditions of labour and the cost of living."

Acting in pursuance of the Board's resolution, the Office conceived its commission to be: "(1) to ascertain the actual facts as regards production and prices; (2) to determine the essential factors which explain the facts; (3) to indicate the solutions which appear to be suggested both by theoretical enquiries and by the facts themselves." No factor relating to underproduction was to be neglected. The task in these dimensions would have seemed Utopian to those in charge had it not been possible to use the results of numerous investigations along similar lines. In the opinion of the Labour Office the task was simply to assemble this information and to supplement it as might be necessary by independent original enquiry.

The Introductory Memorandum is designed to acquaint the public with the general character of the enquiry—"to bring out the spirit of the work and to indicate its main lines and their justification." The extent to which the Memorandum effects this purpose will appear upon a closer examination of the character of the publication.

The Memorandum is really in three sections: a text constituting slightly more than one-half the issue; a schematic plan of enquiry with questionnaires, covering fifteen pages; and a series of fifteen appendices reproducing various documents which, presumably, the Office has found useful in pursuit of the enquiry.

The appendices may be dismissed with a word. They vary widely in length and character. The first is a single-page account of the establishment of the British Government Committee on Increased Production in Industry; the second, a reprint of the questionnaire issued by this Committee. In other appendices questionnaires employed by other investigating bodies are reproduced in full. Appendix XIII reprints the Economic Declaration of the Supreme Council as approved on March 8, 1920; Appendix XII, a report submitted to the Fourth Inter-Allied Commission for Assisting the Disabled in the War, giving statistics relating to the effect of war accidents on the state of employment. Some of these reports furnish valuable material, relevant to an enquiry into production. Others are of doubtful applicability. In general, the wisdom of their inclusion in this Introductory Memorandum may be seriously questioned. It would have been much better to defer their publication until the progress of the Enquiry demonstrated their importance to the ultimate findings.

The text of the Memorandum appears in three parts: Part I: The Facts—the phenomena of physical production; Part II: Explanation of the Facts—the causes of underproduction; Part III: A Critical Study of the Solutions—the means of restoring normal production.

The "facts," submitted in Part I, relate to aggregate amount of production, production per worker, variations in demand, and changes in prices. The only statistics of production actually presented are: (1) those on coal, pig iron, and crude steel, published in the *Monthly Bulletin of Statistics* of the Supreme Economic Council; (2) and the familiar figures on shipbuilding issued quarterly by Lloyds. These are given in detail, in both table and chart form. On the subject of production per worker, the Memorandum reprints data from the report of the United States Bureau of Mines on the daily and annual coal production per man employed underground in numerous important coal-producing countries from 1901 to 1918. No data are submitted on variations in demand. Retail prices of food are shown by reproducing the material presented to the Brussels International Financial Conference by Professor A. L. Bowley.

Inadequate as this collection of material is, it is less unsatisfactory than the data brought together in Parts II and III on "causes," and "solutions." Among the causes cited are: the crisis in raw materials; the transport crisis; the shortage of plant; the exchange crisis; credit problems; numerous factors connected with labor; and psychological and moral factors. The solutions considered relate to: the democratization of industry; the question of piece-work rates; the specialization of labor; the improvement of apparatus; rates of exchange and raw materials. In general, the material on these subjects consists of statements taken from miscellaneous sources, with no attempt at evaluation or interpretation. No better characterization of the presentation can be given than the following quotation: "Whatever may be the qualifications of the author from whom we have borrowed these figures, we reproduce them here without expressing any personal judgment in regard to them."

The outline Plan of Enquiry and Questionnaires, constituting the second section of the Memorandum, is its least objectionable feature. True, the questionnaires are of such a nature as to solicit opinions rather than concrete data, and are in parts so general as to promise no satisfactory results. But the circulation of questionnaires in provisional form has its advantages. And the publication of this part of the Memorandum could be readily understood if its purpose were to secure for the Office the benefit of outside criticism.

There is no specific evidence, however, that any part of the Memorandum has been prepared with this end in view. Upon the whole, the report seems intended to prove the willingness of the Office to include in its Enquiry any item of information however remotely connected with production. There is a consistent indisposition to separate the significant from the irrelevant, the valuable from the worthless, the authoritative from the casual. The recurring promises of what the Enquiry *will* do are not to be taken seriously until there is more evidence of the Office's capacity for judicious discrimination.

Altogether, the Introductory Memorandum is an amazing document. It is a strange conglomeration of material—opinions, tables, and charts, good, bad, and

indifferent. The body of the text is a mixture of plans which it is stated the enquiry will develop; of facts which are submitted in evidence in the study; and of inadequately supported statements drawn from every variety of source. The Memorandum as a whole is of a piece only in its extraordinary deficiencies—its careless composition, its superficial treatment of material, its unintelligent plan of procedure.

The events of the past year add a touch of irony to the entire issue. The crisis of underproduction has passed into a world-wide depression in which traders cannot dispose of goods on hand and manufacturers complain of accumulated stocks. An enquiry into production is needed as much as ever, but it should not be conducted along the lines nor in the spirit of the Labour Office's Introductory Memorandum. This ill-fated publication is an excellent example of what not to print. A fitting title would be: "Preliminary Office Notes—(Published by Mistake)."

EDMUND E. DAY

The Assessment of Physical Fitness. By Georges Dreyer, G.B.E., M.A., M.D. Cassell and Company, Limited: London, New York, Toronto, and Melbourne. 1920. 115 pp.

Dreyer commences his book with a statement that most of the existing tables dealing with the size of the normal human body are based on the theory that definite relations between age and height and weight exist. It has been satisfactorily proved that such relationships do not exist when individuals varying widely in size are examined. Dreyer's findings seem to indicate that definite relationships between weight of body and length of trunk (*i. e.*, the sitting height) and the circumference of the chest do exist, and that these have a definite relation to the vital capacity of the lungs.

He had previously determined these definite relations, not only between the weight and certain measurements of the body but also between these bodily measurements and the functional measurement of vital capacity. and presents his results with a hope that general application of these relationships might add greatly to our present knowledge regarding standards of health development, good physique, and physical fitness. After a careful examination of many thousand individuals, and an analysis of the results obtained, Dreyer is enabled to explain the relationship between weight and sitting height, weight and chest measurement, chest measurement and sitting height, vital capacity and weight, vital capacity and sitting height, and vital capacity and chest measurement, for both males and females, in the following formulae: *

MALES

$$W = 0.38025\,\lambda^{\frac{1}{0.319}};\quad W = 0.662\,Ch.^{\frac{1}{0.365}};\quad Ch. = \frac{\lambda^{1.1442}}{2.00148};$$

* W = weight of the body in grammes; λ = length of the trunk in centimetres; $Ch.$ = circumference of the chest in centimetres; *V. C.* = Vital Capacity in cubic centimetres. The constants for Vital Capacity represent Class A.

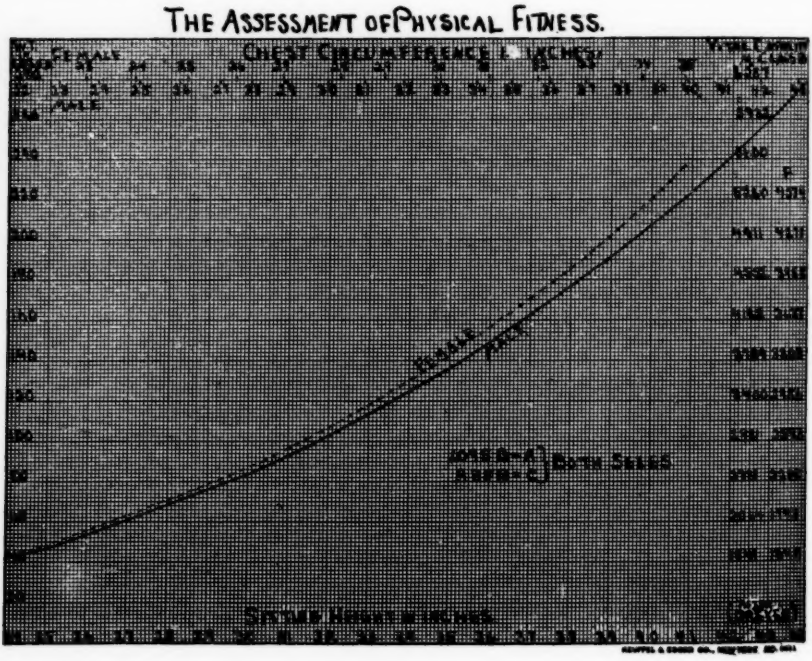
$$V.C.=\frac{W^{0.72}}{0.69};\quad V.C.=\frac{\lambda^{2.257}}{6.1172};\quad V.C.=\frac{Ch.^{1.973}}{1.5595}.$$

FEMALES

$$W=0.36093\times\sqrt[0.313]{\lambda};\quad W=0.30213\times\sqrt[0.284]{Ch.};\quad Ch.=\frac{\lambda^{0.9074}}{0.761833};$$

$$V.C.=\frac{W^{0.72}}{0.79348};\quad V.C.=\frac{\lambda^{2.3003}}{8.2714};\quad V.C.=\frac{Ch.^{2.5352}}{16.4951}.$$

The accompanying graph gives the substance of some ninety pages of tables which furnish information regarding the weight of the human body in its relationship to the sitting height, the chest circumference, and the vital capacity of the lungs. In this graph, English measures are used, except that vital capacity is expressed in cubic centimetres.



To make proper use of the graph, it is essential that all measurements should be taken uniformly, and as nearly as possible in the following manner:

WEIGHT. All weights should be taken as of the body without clothing.

LENGTH OF TRUNK. This should show the distance from the base of the spine to the top of the head. The subject's knees should be elevated sufficiently, the back should be straightened, and the eyes should look straight forward in such a manner that the exact length of vital stem may be obtained.

CIRCUMFERENCE OF CHEST. This should give as nearly as possible the measure taken by a tape in direct contact with the skin. In the males the tape should be held at the level of the fourth intercostal space in the nipple line. In the females this measure is taken at the same level, just under the breasts. At time of taking chest measurement there should be normal breathing.

VITAL CAPACITY. This is a measure of the maximum amount of air an individual is able to expel from the lung by voluntary effort, after taking the deepest possible inspiration. Record the best measure out of five trials. Dreyer finds the most suitable apparatus for this measurement to be made by Georges Boullite of Paris. It will be noted that there is a separate graph for males and for females. Such a division is essential because the two sexes differ in the particular measurements taken.

The tables are intended to be used for the following purposes:

1. The determination of what are the normal proportions between the weight, the trunk-length, and the circumference of the chest.
2. To gain evidence as to underfeeding or malnutrition during different stages of adolescent or adult life, as well as in various classes and occupations of the population.
3. For the study of the different aspects of physical fitness as measured by vital capacity in its relation to weight, trunk-length, and chest-circumference; for the comparison of adolescents with adults, and of the male sex with the female; for the comparison of different trades, occupations, and classes one with another, referring all to a definite common standard.
4. The application of these various measurements to patients with organic disease—*e. g.*, pulmonary tuberculosis—as well as to persons with functional disorders—*e. g.*, industrial fatigue, the fatigue of aviators, and so on.

To gain information concerning these various points, it is obvious that the tables must be used in the correct manner. To ensure this, a detailed description of their use, application, and limitations is now given.

To determine whether the weight of an individual is normal, it is necessary to take his sitting height and then to look up the weight corresponding to that height in the graph, or, in case Dreyer's work is available, in his tables. After taking the circumference of the chest, look up in the graph, or tables, the corresponding weight for that chest measurement. An average of these two weights should give within 5 per cent of the individual's true weight, providing he has a normal weight. For example, if the man's sitting height is 34 inches, he should weigh 125 pounds. If his chest circumference is 32½ inches he should weigh 127 pounds. The average of these two weights, 126 pounds, represents what he should weigh normally. A variation of

Less than 5 per cent indicates normal weight;
More than 5 per cent indicates possibly abnormal weight;
More than 10 per cent indicates probably abnormal weight;
More than 15 per cent indicates certainly abnormal weight in whatever direction the variation may be.

If the examinee has a normal weight in relation to his sitting height, but an abnormal weight in relation to his chest circumference, this condition indicates that his chest is either too large or too small in proportion to sitting height and weight.

For vital capacity, three readings are given under classes A, B, and C. The

readings for Class B only are given in the accompanying graph. Class A includes trained army and navy men, policemen, athletes and active sportsmen, university students and boys in public schools engaged in playing games, members of the fire brigade, blacksmiths and boiler makers. The vital capacity of this class can be computed by multiplying the Class B readings by 1.095.

Class B includes professional classes, business men, railway men, high grade mechanics, the better class of school children, and clerks of the upper class.

Class C includes tailors, shop keepers, shoemakers, printers, potters, lower class clerks, painters, elementary school children, and factory children. The expected vital capacity for this class can be found by multiplying the Class B readings by 0.935.

If a person is found to have as much as 10 per cent less vital capacity than that indicated in the graph or in Dreyer's tables, it is possible that he is suffering from some health-depressing condition. If his vital capacity is as much as 15 per cent below that indicated in the graph, it is practically certain that he is subnormal in this condition. The method of determining what the vital capacity of an individual should be is exactly the same as that for finding what his normal weight should be. Quoting from the author again:

> "It is perhaps worth recording that changes in the vital capacity appear to offer an unusually trustworthy index of any improvement or deterioration taking place in the pulmonary lesions of patients with tuberculosis of the lungs. It also appears from the study of a tolerably large number of such cases that determinations of the vital capacity and its changes may be of value in prognosis, and help the physician to decide, for example, which patients are likely to benefit by further sanatorium treatment and which are not."

The author appears in this work to have supplied physical examiners with a method easily applied whereby physical fitness can be assessed on the basis of four very simple physical measurements, namely, that of sitting height, chest measurements, weight, and capacity of lungs.

FRANK M. PHILLIPS

George Washington University

Defects Found in Drafted Men. By A. G. Love and C. B. Davenport. Surgeon General's Office. New ed. Washington: Government Printing Office. 1920. 1559 pp.

The statistical reports of the results of examinations of American youths (ages 21 to 31) subject to the selective service, which were given out at the time of the examinations, were very instructive and enlightening. The present comprehensive volume deepens and strengthens the impression made by these reports, namely, that we had been negligent about cultivating and conserving the health and strength of our boys; it may also give more accurate statements, and it unquestionably adds materially to the information supplied. Much of this additional information is perhaps of little actual value, exhibiting slight chance variations between different sections of the country; but part of it is indeed illuminating.

The data embraced about 500,000 men rejected for the service, for health reasons, and about 2,000,000 men examined at mobilization camps. These constitute about four-fifths of all those examined; and, as the introduction puts it, they are "representative of all."

The general showing is an average of 468 persons per 1,000 examined defective in some way; on the average, these 468 show 557 defects, many persons being defective in two or more particulars. No less than 208 per 1,000 had some defect of the "bones, joints and the appendages of the hands and feet"; of these, 124 had weak feet and 50 had deformed or injured "appendages." Hernia was found in 40 out of each 1,000 cases; in 5½, spinal curvature.

The number of defects was found to be largest in Rhode Island, where the average was 802 per 1,000 persons; Vermont was second, with 764 per 1,000; the lowest number of defects per 1,000 persons was in Kansas, namely, 422.

Rhode Island was highest also in the number of defective men per 1,000 examined, namely, 640; but only 424 of these on the average were so defective as to cause rejection. This, however, was the largest number rejected per 1,000.

The lowest number of defective men per 1,000 was found in Kansas, namely, 354. Two states, however, had fewer men rejected than Kansas: Wyoming had an average of 128 rejected out of each 1,000 examined, and Nebraska, an average of 134; while in Kansas 147 per 1,000 were rejected.

Of the cities, Denver led in the number of defects, having 800 per 1,000 examined; but the compiler, no doubt justly, ascribes this "to selective gathering there of persons affected with active or latent tuberculosis." The cities in general, however, showed a much higher average than the rural districts, viz., 609 defects per 1,000 examined as compared with 528 in the rural districts.

The states (other than Rhode Island) with more than 500 defective men per 1,000 examined (viz., from 516 to 640) were Oregon, Washington, California, Colorado, Florida, Virginia, Vermont, Massachusetts, and Maine. The states with fewer than 425 (viz., 355 to 425) were Arizona, Texas, Arkansas, Kansas, Nebraska, South Dakota, Minnesota, Iowa, Indiana, Ohio, Kentucky, and South Carolina. It is noteworthy that there was no uniformity of figures in the north, south, east, or west; all four sections were represented in each class.

The view that Denver's very high ratio of defects is due to the incurrence of many tuberculous youths from other sections is upheld by the percentages of rejections for tuberculosis, which are highest in the states resorted to by tuberculous persons. The percentages are as follows:

	Per 1,000 Examined		
Arizona	64	Colorado	49
New Mexico	61	California	43

States similarly situated but not used as resorts are among the lowest on the list, all having a record of 15 or under:

	Per 1,000 Examined		
Wyoming	10	Utah	13
Montana	11	Idaho	14
Nebraska	11	Iowa	15
North Dakota	11	West Virginia	15
Delaware	13	South Dakota	15

With regard to venereal diseases, the largest rates are entirely in states in which the proportion of blacks to whites is greatest:

Over 50 per 1,000 Examined		Under 25 per 1,000 Examined	
Florida	163	Vermont	13
Georgia	136	South Dakota	16
Mississippi	132	New Hampshire	18
South Carolina	131	North Dakota	19
Louisiana	121	Utah	19
Alabama	115	Wisconsin	20
Texas	112	Alaska	21
Arkansas	105	Oregon	22
Oklahoma	85	Idaho	22
District of Columbia	81	Wyoming	23
Delaware	77	Massachusetts	24
Virginia	71	Maine	24
North Carolina	70	Minnesota	24
New Mexico	67	Colorado	25
Maryland	67		
Missouri	65		
Tennessee	65		
Illinois	53		
West Virginia	53		

The record with regard to cases of syphilis per 1,000 examined is much the same:

Over 10 per 1,000 Examined		Under 5 per 1,000 Examined	
Alabama	38	Idaho	1
Louisiana	33	Vermont	2
Georgia	29	Oregon	2
Delaware	24	Wisconsin	3
District of Columbia	23	New Hampshire	3
Oklahoma	23	South Dakota	3
Florida	22	Maine	3
Mississippi	22	North Dakota	3
Arkansas	20	Utah	3
Maryland	17	Massachusetts	4
Missouri	17	Colorado	4
South Carolina	16	Washington	4
North Carolina	16	Rhode Island	4
Illinois	14	New Jersey	5
Indiana	10	Alaska	5
Tennessee	10	Connecticut	5
		New York	5

In both tables the figures are the nearest whole numbers to the numbers given in the report; where the numbers given equal the limiting number, they represent fractional numbers lower than the limit by less than one-half.

In the public press it was reported not long ago that Utah had the lowest percentage of venereal diseases and of syphilis. This proves not to be the case. Vermont has the lowest percentage of venereal diseases, with a record of 13 per 1,000 examined, while Utah is fifth lowest, with a record of 19 per 1,000; with regard to syphilis, Idaho leads, with a record of but 1 per 1,000; while Utah is ninth, with a record of 3 per 1,000.

For the whole country the average of tuberculous persons was 30 per 1,000 examined; of those with venereal diseases, 32 per 1,000. Tuberculosis was less common among country boys, taking the average throughout the country.

For the whole country, the number per 1,000 examined found to be afflicted with venereal disease was not materially lower for the country than for the cities. Another interesting statement is that in the whole country 1,488 cases of drug addiction were found, as against only 853 of alcoholism.

With regard to mental deficiency, the highest ratios, running from 16 to 31 per 1,000 examined, were found in states in which some part of the population

has been shut off, largely interbreeding; these are Louisiana, Mississippi, South Carolina, North Carolina, Virginia, Vermont, Maine.

The analysis of the experience in New York state by sections shows:

With regard to tuberculosis, the highest is in the Adirondack region, a resort for consumptives; the lowest is in the suburbs of New York City.

With regard to venereal diseases, the highest is in the "western manufacturing section," including Buffalo; the lowest is again in the suburbs of New York City, with the city itself a close second.

With regard to mental defects, the highest is in the Catskill section, with its old, long-settled, intermarrying regions, while the very lowest is in New York City. The Catskill section has also the highest percentage of epilepsy.

MILES M. DAWSON

The Problem of Estimation. By Correa Moylan Walsh. London: P. S. King & Son. 144 pp.

This little monograph, by the author of *The Measurement of General Exchange Value*, will be welcomed by all students of index numbers. It will doubtless be read by many such who have not been willing to devote the greater time required for reading the author's earlier and larger book. The present volume does in fact include the gist of the larger work besides adding the author's later and maturer conclusions. The earlier book, *The Measurement of General Exchange Value*, is the most elaborate and scholarly book on index numbers that has yet appeared and it has therefore become a classic in the literature of that subject; although, because of its minute and technical character, it has inevitably had very few readers.

Having read and reread both books I am filled with admiration for the author's keenness and for his painstaking industry. He has covered very thoroughly the antecedent literature and has worked out the details and the peculiarities of many formulae and methods. Inasmuch as the author does not consider himself a mathematician, his thoroughness and penetration in what is, after all, a mathematical field, seem remarkable.

If I were to offer any adverse criticism it would be chiefly that the method of the writer seeks too much to lead the reader from the consideration of special cases up to general cases, instead of the reverse. In some instances, at least, he could have made it easier for the reader, if not for himself, had he proceeded in the opposite direction. As J. Willard Gibbs, the mathematical physicist used to say, "the whole is simpler than its parts."

It should be stated that *The Problem of Estimation* is more than a treatise on index numbers. The subject of index numbers proper occupies only the last fifty pages. The other ninety pages will be of less interest to the economist, although they help materially to introduce the author's presentation of the theme of index numbers.

The book consists of an Introduction in which "the controversy" is formulated, a second part on "Averages and Errors," and a third part on "Deviations and Variations."

The "controversy" is presented rather entertainingly by propounding an old problem treated by Galileo and others of his time: "If a horse worth one hundred crowns is estimated by one person at one thousand and by another at ten, which of these two estimates is the less erroneous, or are they equally erroneous?" Galileo maintains that the two guesses are equally erroneous, while a certain Nozzolini stoutly argues that 10 is nearer the mark (100) than is 1,000. This leads naturally to a discussion of the arithmetic and geometric means. Geometrically, ten and one thousand are equally distant from one hundred, one being ten times as much and the other ten times as little, whereas, arithmetically, ten is nearer one hundred than one thousand, being only distant by a difference of ninety as compared with the other difference of nine hundred.

As in his former work, the author devotes most of his effort to contrasting the advantages and disadvantages, for various purposes, of the geometric, arithmetic, and harmonic means. He offers various criticisms—not always very important—of other writers on index numbers, including Edgeworth, Bowley, and myself.

One of the parts of the book which most interested me, personally, is that concerning the formula on page 102. This is the formula which I have offered in the March number of the QUARTERLY PUBLICATIONS as the best formula for an index number. Walsh, from an entirely different approach, has reached the same conclusion. The particular merit of this formula, which he seems to regard as the most important, is quite apart from the merits which I have brought forward. It is that this formula comes nearest to fulfilling "Westergaard's test." No formula has been found which completely satisfied Westergaard's test (except formulae with obviously improper weighting). This so-called test of a good index number is that, by its use, a comparison between two years *directly*, such as 1913 and 1921, ought to correspond to the result obtained *indirectly*, by putting together a chain of index numbers for intervening years. Although the formula which he and I agree is the best does not perfectly fulfil Westergaard's test, he thinks it comes nearer to it than any rival formula.

It was evidently the hope of finding the formula that would fulfil Westergaard's test which has twice called Walsh into this field of research. He is disappointed not to have found the object of his search, and expresses a suspicion that perhaps the problem, like squaring the circle, is after all insoluble.

As I stated in my March paper, I hope to show later that this suspicion is well grounded and, furthermore, that Westergaard's test is not a true test at all, but presents an ideal which is not only unattainable but false.

But just as the search for a method of squaring the circle or discovering "the north-west passage" were by no means fruitless, so Walsh's research has reached many results of value to science, especially to the science of Index Numbers.

IRVING FISHER

Rules for Reporting Information on Railroad Employees, together with a Classification and Index of Steam Railroad Occupations. Reporting forms and classification plan, prepared by the United States Railroad Labor Board, and approved by the Interstate Commerce Commission, to be used by carriers

in reporting wage and compensation data to the United States Railroad Labor Board and to the Interstate Commerce Commission. (To be effective July 1, 1921, superseding Order June 17, 1915, and supplementary Order July 13, 1917.) May, 1921. 320 pp.

The introductory statement to the report gives at the outset the following brief description of its seven main divisions:

1. Statement on the present reporting plan, and the purposes, character and uses of the revised occupational classification of railroad positions
2. Rules governing the classification of steam railway employees and reports of their service and compensation
3. Reporting forms to be used by steam railroads in reporting information on railroad employees to the Interstate Commerce Commission and to the United States Railroad Labor Board
4. Detail on Reporting Divisions contained in the forms for reporting wage and other information by steam railroads to the Interstate Commerce Commission and to the United States Railroad Labor Board
5. Outline of the occupational classification of railroad positions
6. Occupational classification of railroad positions
7. Alphabetical finding list of illustrative payroll and distinctive class titles of railroad positions

Typical of the common experience with occupational classifications is the history of the federal classification of railway positions. The early classification was a simple affair with 18 occupational groupings. On July 1, 1915, a revised classification was adopted providing for the separation of occupations into 68 groups, and it has served as the basis for the statistics since published by the Interstate Commerce Commission on service and compensation. When the attempt was made to use this classification for the solution of concrete problems of personnel administration and control it was found too general. The report says:

"During the period of federal control the United States Railroad Administration found it difficult in the adjustment of wage and other disputes to secure the type of statistical information as to service and compensation of railroad employees which it required. The Railroad Wage Commission in circulating questionnaires as a basis for the first award—General Order No. 27—used primarily the sixty-eight (68) groupings of occupations in effect at the time.

The Board of Railroad Wages and Working Conditions, appointed by the Railroad Administration, experienced the same difficulty that was encountered by the Railroad Wage Commission in using the data on wages compiled in accordance with the groupings prescribed by the Interstate Commerce Commission. Accordingly, in special cases, elaborate questionnaires were circulated to secure the desired information. Due to the difficulty experienced by it, the Railroad Administration submitted to the Interstate Commerce Commission for its approval a proposed classification of railroad occupations embodying one hundred and twenty-five (125) distinctive groupings of occupations and with it report forms for reporting both service and compensation according to these distinctive groups. These report forms provided not only for expanding the occupational groupings from 68 to 125, but also provided that employees' services should be separated as between overtime, straight time, special allowances, etc. It also provided that compensation should be similarly distributed.

Meanwhile, under the Transportation Act, 1920, the United States Railroad Labor Board was created. This Board inherited a great number of requests for the adjustment of disputes between employers and employees and such an adjustment was provided by the Board in Decision No. 2. In deciding the questions at issue, however, the Board was seriously handicapped because of the absence of wage data relating to homogeneous groups of occupations and classified according to the special working conditions found in railway operation. The absence of data was in part

compensated for by the mass of information which was inherited from the Board of Railroad Wages and Working Conditions and without which it would have been difficult, if not impossible, properly to review the claims of the contending parties. Provisions for the current collection of similar data, however, had not been made and it seemed evident that steps would have to be taken by the United States Railroad Labor Board to secure such current and properly classified data if it were correctly to perform its functions according to the provisions of the law."

Before the classification with 125 groupings as proposed by the Railroad Administration had been officially adopted by the Interstate Commerce Commission, the Railroad Labor Board was created, and it requested the Commission to defer approval of this classification until the Board had had time to study the subject in relation to its particular problems and purposes.

The new classification is the result of this study. It recognizes 500 distinctive classes of positions, in addition to the positions in "the Executive, Official, and Staff Assistant Service." "These classes are the smallest units provided for in the classification and are made up of positions carrying essentially similar duties and responsibilities. Each class is given a distinctive title and described so far as is necessary to indicate the kinds of positions which fall under such distinctive class." For purposes of reporting statistics the 500 "classes" and the executive, official, and staff assistants are grouped into 148 divisions, most of the divisions containing a number of fairly homogeneous "classes."

In spite of the 500 classes and an almost inevitable demand for simplicity, one may hazard the prediction that the tendency will be toward an increase in the number of classes rather than toward a decrease. There is no royal road to simplicity. The degree of diversity of the occupations in the industry must determine the number of classes if the classification is to be a useful, practical instrument for improving the conditions of employment and the relationship between employer and employee.

The Railroad Labor Board and the Interstate Commerce Commission are to be congratulated on their decision to go to the fundamental unit, the class of positions with like duties and responsibilities, in the presentation of the classification rather than to give only the divisions into which these classes are to be grouped for the purposes of reporting statistics. The section on the purpose of the classification begins with the statement:

"The purpose of the occupational classification of railroad positions is to furnish a basis for the collection of wage and other data on homogeneous classes of railroad positions, to establish as nearly as can be a uniform terminology to be used in describing similar occupations, and to describe classes of positions so that all railroads may have the same general understanding of the kinds of positions that are to be included in any class."

The introductory statement concludes with the illuminating sentence:

"It is hoped, however, that the classification in its detailed form will prove of such value to the railroads in viewing their employment problems as a whole that they will, of their own free will, install it as a basis for employment administration, and reap the advantages which would accrue therefrom."

In other words, it is another classification that has been broadly conceived and broadly executed.

Against detailed specific classifications an argument is now being advanced

in some quarters that it tends to establish jurisdictional lines for occupations, that employees in one class will object to performing certain duties, even temporarily, because those duties are not enumerated in the description of their class and are enumerated in the description of some other class. The report is specific in its statement that the classification is not to be so construed. It says:

"The occupational classification is in no way and under no circumstances to be interpreted by the Board, by the Interstate Commerce Commission, by the railroads or by other interested parties, as setting up jurisdictional lines for occupations, or as limiting the kinds of work which employees may perform or the duties which they may assume. In preparing the occupational classification, the Board has not aimed to standardize for the railroads the occupational duties assigned to, or the kinds of work performed by their employees, and nothing in the classification nor in the report forms is to be construed in this light."

The admission must be frankly made that a detailed classification probably does tend to bring controversies to an issue. Under a broad general classification facts may be buried or disguised, whereas a detailed classification brings them to light, and it may do so at an embarrassing time. Such situations must, however, be faced and controversies settled; and the more promptly and courageously this is done the better it will be for all concerned. The trouble in the past has been to get the facts. The progress in this direction promised by the new classification and the rules will more than offset any disadvantages that may accrue through any jurisdictional disputes that may arise.

Of the classification proper the report says:

"The basis and general plan of the occupational classification conform to the recognized lines and the best practice which have been followed by industrial and governmental organizations in the development of employment classifications for their services, the entire plan being based upon the principle that duties and responsibilities must control in any orderly arrangement and classification of positions."

To this statement one can readily subscribe, for the work of classification of employments has progressed to the point where there are in broad general matters recognized lines and a best practice, and with these standards the classification seems in general to be in harmony. One misses under the statement of duties and responsibilities any description of the qualifications required for filling the positions, but of course no such statement is necessary for the primary purpose for which the classification is developed. Recruitment and promotion are processes in employment with which the railroads and not the Commission or the Board are primarily concerned. Doubtless railroads which use the classification for personnel administration will find it helpful, in formulating their standardized employment policy, to supplement the classification by adding statements of what they require for entrance into the several classes of positions. In the public service such statements are of more immediate importance than in industrial establishments, but as the truth is learned it will doubtless be that big private enterprises do not differ very radically from the government with respect to what is best in methods of recruiting.

In providing for reports to the Commission and to the Board two forms are to be used, Form B relating to the train and engine service, and Form A to all other employees. Form A requires monthly for each of the 130 reporting divisions of occupations a statement of (1) number of employees in the middle of

the month; (2) number of full time positions (average for the month); (3) service hours (or days) of (a) straight time actually worked, (b) overtime paid for at pro rata rates, (c) overtime paid for at punitive rates, (d) time paid for but not worked, and (e) total time paid for; and (4) compensation for (a) straight time paid for, (b) overtime paid for at pro rata rates, (c) overtime paid for at punitive rates, (d) time paid for but not worked, and (e) total. Form B for the train and engine service calls for the number of employees not only at the middle of the month but also on the 7th, the 22nd, and the 28th, and it omits the number of positions. It calls, broadly speaking, for similar information regarding service hours and compensation, and then asks for miles actually run and miles paid for but not run, and for the total number of trips for which not less than a minimum day was paid.

The report leaves one with the impression that now the country is to get the facts regarding railroad employees and railroad labor.

LEWIS MERIAM

Negro Migration during the War. By Emmett J. Scott. New York: Oxford University Press. 1920. viii, 189 pp. (Carnegie Endowment for International Peace, Preliminary Studies of the War, no. 16.)

Some one, doubtless, in days ahead, will find it worth while to write an account of mobility of labor, geographical and occupational, during the war. Though he will perhaps trace no more extraordinary instance of international migration than the shipment of many thousands of coolies from Asia across Canada and the United States to Europe, he will hardly discover a more impressive case of internal migration than that of the negroes who moved northward in the United States. How many had a part in this movement cannot be said with precision; estimates and vague guesses added together give over 400,000. Their going had all the characteristics of a mass phenomenon, a herd movement, like all the great migrations of history. The rationality of the mass was put above individual calculation. "Drivers and teamsters left their wagons standing in the street. Workers, returning home, scrambled aboard the trains for the North without notifying their employers or their families." The economic bait was the most powerful in its attraction, but the notion of deliverance was also present. Part of this had a deep religious cast: "The movement was called the 'exodus' from its suggestive resemblance to the flight of the Israelites from Egypt, *The Promised Land, Crossing over Jordan* (the Ohio River), and *Beulah Land.*" In part the notion of deliverance was a specific reaction against the oppressive social and economic discriminations of the South. Just how important the latter factor was is made less clear in Mr. Scott's study—perhaps intentionally so—than we could wish.

The author tells us a good deal about the sources of the migration, its course, how (as by letters and labor agents) the attraction was given an opportunity to act, what efforts, lawful and unlawful, were made to check the emigration, how it fared in different parts of the North, what gains were realized, where disillusionment came, how public opinion viewed the whole procedure, and what national remedies might avail.

As a study of the causes and course of emigration, at least its psychological phases, the volume has permanent value. Reading it, one is led to wish that studies of this sort were available for the great European migrations; but none of broad scope exists, I believe, except for Italy and Sweden, both of which are governmental.

In the present book there is nothing noteworthy from a statistical point of view, and some portions are weak. The author, for instance, though realizing that the figures given for the dimensions of the recent movement are of uncertain value, is willing to reproduce uncritically precise figures for some earlier movements which can have rested on a no more certain basis. The bibliographical scholarship is crude: footnotes, for example, refer to periodicals by volume, not stating authorship, which also is not stated in the text, and the items of the appended bibliography of books and periodicals are arranged on no discernible principle. A volume issued by the Division of Economics and History of the Carnegie Endowment for International Peace ought to maintain a higher standard in this respect.

ROBERT F. FOERSTER

Methods and Results of Testing School Children. By Evelyn Dewey, Emily Child, and Beardsley Ruml. E. P. Dutton and Company: New York. 1920. 170 pp.

Before me are two published estimates of this book. Judging by these two comments only, one might infer that the book reviewed here is concerned with some such temperamental topic as relativity or the like. One estimate is given by Fowler D. Brooks, of Johns Hopkins University, and is taken from his doctor's dissertation:

"Dewey, Child, and Ruml have carried out one of the most scientific experimental studies in educational psychology."

The other estimate is by Walter S. Monroe, of the University of Illinois, and is quoted from the *Journal of Educational Research*. Among other somewhat more complimentary remarks, he says:

"This book does not appear to be of major importance. The average school man will find little in it that is helpful."

Both estimates are correct. From the point of view of the advanced student of technical mental measurement and mental statistics, the book is a model. It is in fact probable that the statistical refinements exceed the refinements in the original data. Although there are no original contributions in the way of techniques, with perhaps two minor exceptions, the whole investigation was conceived and executed in accordance with the highest traditions of psychological and statistical methodology. I read the book before I was invited to review it, and immediately placed it upon the list of reference readings for my students of mental measurement and statistics. But to the average school man the significant portions of the book are relatively valueless, primarly because they are incomprehensible. Final evaluation as to immediate utility of the investigation

should be postponed, however, until a promised second volume has appeared. It is probable that this second volume will deal with more practical and popular matters.

The investigation undertaken by Dewey, Child, Ruml and others had several objects. One aim was to select or construct a series of mental tests which would give as complete a picture of a child's mental processes as possible. Very wisely, I believe, the investigators made no attempt to isolate and measure the various interwoven mental traits. As far as possible they tried to rid themselves of any hypothesis concerning the nature and constitution of mental ability. They selected and, in several instances, originated mental tests which measure various practical activity-wholes. Finally, they so standardized the method of applying and scoring each test as to make it quite objective and, in particular, quantitative. A few mental diagnosticians still base their diagnosis upon highly subjective and relatively inexplicable interpretations of a child's subtle behavior hints while taking a test. These investigators, on the other hand, wished not only to base their diagnosis upon objectively and quantitatively scored reactions—to diagnose "by" tests not "from" them—but also to secure a body of data susceptible to statistical manipulation.

A second aim of the investigation was the establishment of age norms for a homogeneous group. They employed an ingenious and, in part, original method for selecting public school pupils so as to secure a representative sampling for each desired age of East Side Jewish children in New York City. The various mental tests were given to one pupil at a time. The mean score for each test for each age became the norm of that age. Equations are presented whereby a norm may be computed for any intervening age.

A third aim was to select, from plans previously originated, a method for reporting a child's score so that it would be interpretable immediately and would be comparable from test to test. Both purposes were achieved by expressing each child's score as a sigma deviation from the norm for his own age for the test in question. It should be noted that this plan makes no provision for making more accurate the original units of measurement, namely, crude score units, nor does it permit a direct comparison between scores made by pupils of one age group and scores made by pupils of another age group. The former is a defect, while the latter is only an inadequacy.

A fourth aim was the preparation of a maturity scale, or, more exactly, a maturity test, since no attempt was made to scale the tests employed. By means of the partial-correlation-and-regression-equation technique those tests were chosen for the maturity scale which had the largest independent diagnostic significance. The criterion employed for determining the independent diagnostic significance of each test was chronological age. This use of chronological age as a sole criterion is something of a surprise in view of the fact that most mental measurers, when preparing an omnibus intelligence test, customarily employ chronological age as only one of several criteria and usually prefer such criteria as: the age of reaching a given school grade; a composite of the several tests used; school marks; estimates of teachers; previously established intelligence tests, etc.; or a weighted composite of one or more of these. The use of chronological

age alone as a criterion might give as much influence to body weight in determining a child's intelligence as to skill in reasoning. The authors foresaw that the validity of their criterion would be attacked. They anticipated such attacks by naming their scale a *maturity* scale, rather than an *intelligence* scale, and by pointing out the consequent limitations. It is significant, however, that one competent reviewer failed to perceive this distinction and wrote as though the maturity scale were an intelligence scale. In my judgment the maturity scale will prove a reasonably satisfactory intelligence scale because the investigators were discriminating workers. Before applying their criterion they partially parried criticism by eliminating those tests which were primarily physical rather than mental. Suffice it to say that chronological age is a satisfactory criterion for maturity tests but not for intelligence tests unless employed with discrimination and unless supplemented by subordinate criteria—more subordinate criteria, in fact, than were employed by Dewey, Child, and Ruml.

A fifth aim was to correlate scores made on the various mental tests with the home environments of the children. This aim failed of realization because of the unusual homogeneity of environments. Generalizations from the social data secured are given, however, in order that future investigators may have fuller knowledge of the children upon whom the test norms were based.

A sixth aim was to correlate the physical records for the children with their mental records. For various reasons an ambitious program of physical measurements was reduced, finally, to measurements of weight, height, head, chest, teeth, and blood pressure. Age norms for these and for certain resulting ratios are presented. But the really best result from this portion of the investigation is summarized in this quotation:

> "The great need of the development of a standardized procedure and terminology for the use of medical examiners in making such mass studies of normal children was very apparent from the analysis of our material."

This is the second investigation of mental and physical relationship I have seen substantially fail because of a belief on the part of non-medical individuals that medical science has devised a system of exact, objective, and quantitative measurement of significant physical conditions. One such investigation I recall very vividly because I myself was the victim of such a misapprehension.

In summary, and in the words of the authors:

> "This volume furnishes a guide for workers who may wish to use the methods employed here for clinical work or for further investigation. The tests with their instructions and norms, the maturity scale with an explanation of its meaning and use, and a summary of the social and physical studies are presented. A further publication will deal with the possibilities of interpreting the objective data of mental tests. An analysis will be made of relationships found for the physical, school, and test measures, from the point of view of the practical problems presented by school children."

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